Psychological Bulletin

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Psychological Bulletin

BRAIN WAVES AND PROBLEMS OF PSYCHOLOGY[1]

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Since the war clinical and experimental electroencephalography have flourished. Studies employing EEG techniques have revealed much about electrophysiological mechanisms associated with normal and abnormal brain functions. The purpose of this paper is to summarize recent experimental findings and hypotheses arising from them, which pertain to problems of traditional interest to psychologists.

In his chapter on electroencephalography in Hunt's 1944 handbook (121) Lindsley reviewed the then-known relationships between EEG phenomena and physiological and psychological processes. A knowledge of the fundamentals as presented by Lindsley will be assumed in this paper, and material which appeared in his chapter will not be reviewed again. Material which does not at present appear to be related to psychological functions will also be omitted.

SLEEP AND WAKEFULNESS

In 1939, after an exhaustive review of the literature on sleep and

wakefulness, Kleitman (99) concluded that, although the flow of sensory impulses into the brain is instrumental in initiating and maintaining the state of wakefulness, the brain mechanisms involved cannot be as simple as might first appear. Activity of cortical origin must be capable of maintaining the waking state in the absence of heavy afferent inflow. Kleitman called wakefulness maintained by sensory inflow "wakefulness of necessity," and that maintained by cortical activity "wakefulness of choice." On the basis of the pathological and experimental data at his disposal, he concluded that both sensory-afferent and cortical influences must work through a "wakefulness center" in the brain, which is probably located in the hypothalamus and perhaps extends into the thalamus and midbrain, and further, that as long as the wakefulness center is active it maintains the waking state by its influence on the brain as a whole. He recently reiterated these conclusions (100).

Experiments by Magoun, Lindsley, and their colleagues indicate that the brain stem reticular formation (BSRF) fulfills the requirements of a wakefulness center as postulated by Kleitman. The BSRF is located ventromedially in the brain stem and extends from the medulla through the pons, midbrain, sub- and hypo-

thalamus, and into the ventromedial thalamus. Thus it occupies the approximate position predicted by Kleitman to contain the wakefulness center, but is somewhat more extensive than he predicted (133).

In some of the experiments of Magoun *et al.* observations of EEG[2] changes were substituted for observations of behavioral changes because of the close relationship between particular EEG patterns and the various stages of the sleep-wakefulness cycle—alertness, relaxation, drowsiness, and light and deep sleep (121). The general scheme and sequence of patterns is very similar among the higher vertebrate species. The EEG of an animal drowsing or asleep is "hypersynchronized," that is, characterized by the presence of distinct slow waves (slower than 8 cps in man) and frequently also rhythmic faster activity (sleep "spindles"). When the animal is awake, but relaxed, the EEG is dominated by a rhythm of intermediate frequency (8–12 cps in adult humans, the well-known alpha rhythm). When the animal is alert the EEG is desynchronized, that is, rhythmic activity tends to disappear and is replaced by low-voltage fast (beta) activity. Magoun and his associates have used the term *activation pattern* to describe the desynchronized EEG and have called behavioral arousal or alertness *behavioral activation* (126, 127). These terms will be used in the following discussion.

Direct electrical stimulation of the BSRF in relaxed or somnolent cats results in activation of both the EEG and behavior (141). On the other

[2] For simplicity's sake the term EEG will be used in this paper to refer to all recordings of brain-electrical activity, whether rhythmic or transient, and regardless of the location of the recording electrodes.

hand, progressively more rostrad sectioning of the BSRF results in the progressively more prominent appearance in the EEG of hypersynchronous activity, very similar to that seen in normal sleep, with corresponding behavioral somnolence (126). After bilateral lesions of the hypothalamus, through which the ascending neuronal relays funnel (175), hypersynchronous EEG activity is persistent and can be recorded both from the cortex and the intralaminar nuclei of the thalamus. This effect of hypothalamic lesion can be demonstrated unilaterally: if the lesion is confined to one side of the hypothalamus, hypersynchronous activity persists only over the ipsilateral hemisphere. Chronic cats with bilateral hypothalamic lesions, or with rostral-midbrain section of BSRF, remain somnolent as long as they survive, and hypersynchronous activity persists in their EEG's (127). If some of the BSRF is spared however, there is a tendency for EEG patterns to return to normal with time (107).

It is clear from these results that the BSRF is essential to the maintenance of the waking state under normal conditions. The next problem is that of the relationship of reticular activity to the afferent sensory activity which is capable of effecting the arousal of a somnolent animal under normal conditions.

If sensory end organs or peripheral nerves are stimulated, specific electrical responses can be recorded in the BSRF but not in adjacent structures other than the classical sensory pathways and relay nuclei (176). This indicates the existence of collateral innervation of the BSRF from the long sensory afferent tracts (the medial and lateral lemnisci, etc.) as they pass the reticular formation

in the brain stem, especially in the region of the medulla, pons, and midbrain.

After transection of the BSRF, strong sensory stimulation results in transient behavioral and EEG activation, but such activation outlasts the stimulus by only a few seconds (126). Therefore, sensory impulses arriving in the forebrain (thalamus and cortex) can initiate the state of arousal, but arousal cannot be maintained in the absence of the BSRF.

Stimulation of animals with intact BSRF's, but with interruption of all sensory tracts in the midbrain rostrad of the entry of collaterals to the BSRF, results in prolonged EEG and behavioral activation, even though no arriving impulses can be detected in the appropriate sensory area of the cortex and specific behavioral responses to the stimuli do not occur (126). Animals in a chronic condition such as that just described continue to exhibit sleep-wakefulness cycles both behaviorally and EEG-wise (127). Thus it appears that the arrival of sensory impulses at the cortex is not essential to the initiation or maintenance of wakefulness or to the sleep-wakefulness cycle as long as the BSRF and its corticipetal projections are intact and as long as sensory pathways are patent as far as the level of the BSRF.

The experiments described thus far were performed on cats. Some of them have been repeated on monkeys with the same results (53, 54), except that the latter do not survive complete destruction of the cephalic portion of the brain stem. But in monkeys even incomplete lesions resulted in more profound somnolence and greater EEG hypersynchrony than was observed in cats. Unless much of the reticular substance was spared,

even intense peripheral stimulation failed to elicit either behavioral or EEG activation. In man clinical observations suggest that destruction of the reticular system of the brain stem or its isolation from higher functional areas results in prolonged somnolence, just as do similar experimental lesions in cat and monkey (51, 88, 99, 179).

Taken together these findings indicate that a background of maintained activity in the BSRF accounts for the maintenance of wakefulness, while reduction of its activity precipitates a state of somnolence or unconsciousness. The BSRF mediates the activation of the forebrain which is associated with alertness following sensory stimulation. It is a moot question whether consciousness as a psychological state, in man at least, is possible without the cortex. It is likely that the BSRF constitutes a wakefulness center because of its influence upon the activity of the cortex. Further, in accordance with Kleitman's hypothesis it would be expected that impulses from the cortex, as well as those from peripheral receptors, can excite the reticular formation, resulting in what Kleitman calls "wakefulness of choice." This has recently been demonstrated by French *et al.* (52) in the monkey, where it was shown that stimulation of certain cortical areas produced electrical responses in the BSRF.

The role of the thalamus in sleep has also received attention. During sleep, bursts of mixed fast and slow electrical waves can be recorded almost simultaneously from the thalamus and cortex (126). While a definite interrelationship between such cortical and thalamic activity appears to obtain under normal conditions, burst activity in those structures is in reality at least semi-inde-

pendent (**44, 82, 91, 105, 113**), and whether the thalamus dominates the cortex during sleep is an open question. Some indication that it may do so is found in reports of the experimental induction of sleep by electrical stimulation of certain thalamic nuclei (**3, 75, 76, 136**).

The picture which emerges then is this: In the sleeping animal large areas of the cerebral cortex are inundated by great waves of electrical activity, thousands of neurons beating in synchrony. A diffusely projecting thalamic system participates in this activity. Afferent sensory impulses can reach the primary cortex, but their spread appears to be blocked. The BSRF is relatively inactive, and apparently relatively insensitive to afferent sensory impulses. When the flow of afferent impulses (number of impulses per unit time) becomes supraliminal, or when the excitability threshold of the BSRF is reduced, as must occur for example at the end of a night's sleep, the BSRF is stimulated to activity and initiates a flow of impulses into the forebrain. The hypersynchronous electrical activity of the forebrain is thereby arrested and is replaced by low-voltage fast activity and alpha waves, as is seen in the EEG. Sensory impulses are again received, transmitted, and integrated in the cortex. The animal is awake and alert.

This formulation leaves some major questions unanswered—such as what factors are responsible for the regular, but alterable, cyclic variation of such processes in time—but the central nervous mechanisms of the cycle have been greatly elucidated by these studies.

SENSATION

The onset, and often the "offset," of a stimulus may be followed by one or both of two classes of cortical responses: (*a*) transient evoked potentials, and (*b*) changes in the background activity pattern.

Evoked Potentials in Man

Recording evoked cortical potential changes from the scalp is not as satisfactory as recording them from the exposed cortex, but in man it is the only available technique, except at the time of exploratory brain surgery. Such recording from the scalp has however been repeatedly demonstrated, not only using light flashes as stimuli (**64, 137, 157**), but also using somesthetic (**39**) and auditory (**121**) stimuli and following direct electrical stimulation of peripheral nerve (**38, 40, 115**). Cathode ray oscillographs are usually necessary to demonstrate such phenomena (**38, 137**), but some evoked potentials can be recorded using conventional EEG amplifying systems (**28, 48, 49, 55, 158**).

The method can be used in determining afferent transmission time in reaction-time experiments (**137**); and interaction of cerebral responses to visual and auditory stimuli has been demonstrated (**181**). The latter may be part of the physiological substrate of sensory-interaction phenomena.

The Effects of Repetitive Stimulation

Twenty years ago Adrian and Matthews reported that repetitive visual stimulation ("photic flicker") was capable of "driving" brain rhythms at the frequency of the stimulus. Several subsequent investigators confirmed and extended these observations (**121**). It has been reported that intermittent auditory stimulation too can "drive" brain rhythms, but not nearly as readily as visual (**55, 61**).

There has been a recent revival of intense interest in the use of photic flicker stimulation, stemming largely from the work of W. Grey Walter

(203). As well as being an experimental instrument of interest in neurophysiology and neuropsychology, flicker stimulation has proved useful in epileptology, (**57, 96, 153, 156**), but we will not be concerned with this application.

Stimulation Techniques

Early work was done using a steady light source interrupted by a rotating sectored disk. The frequency of stimulation was varied by varying the rate of rotation of the disk. More recently all-electronic stimulators have come into general use. The light source (stroboscope) is placed a few cm. from the eyes so as to stimulate all areas of the retina uniformly. Typical stimulus values used in EEG work are blue-white light, 88,000 candles, flash duration 15–50 microseconds (**23, 56, 145, 199**), with frequency continuously variable from 1–100 flashes per second (fps). The very brief flash duration, which is constant regardless of frequency, permits the use of light of intensities which would injure the retina at higher light-dark ratios (**191**). Some epileptics are more sensitive to red light than to that of shorter wave lengths or to white light (**23, 191**). For some purposes two or more stimulators have been used simultaneously (**164, 199, 202**).

Recording Techniques

Standard EEG equipment is used to record cerebral activity during stimulation. One channel is used to record the stimulus, which is usually detected by a photoelectric cell.

Two recently developed accessory recording devices make the flicker-stimulation technique an even more powerful experimental tool: the automatic, ink-writing frequency analyzer (**103, 199, 201**), for the data from which Gleser has recently de-

veloped a method of statistical treatment (**60**); and the toposcope (**29, 30, 204, 206**), by means of which the electrical activity of the brain is displayed on a battery of cathode-ray tubes.

Physiological Effects

The simplest brain electrical response to repetitive light stimulation of the retina is the occurrence in neural elements subserving visual functions, or in related structures, of a series of evoked potential fluctuations at the frequency of the stimulus.

The upper and lower frequency limits to which neural elements will follow a repeated stimulus are dependent upon a number of factors. One variable is neural level. Lindsley (**125**) has observed following up to 100 per second in the retina, optic tract, and lateral geniculate of the cat, but only to 40 or 50 per second in the visual cortex. Walker *et al.* (**194**) observed following to 62 per second in the optic nerve, 59 per second in the lateral geniculate, and only to 34 per second in the visual cortex of the macaque.

Gastaut (**56**) recorded both from the subcortical white matter of the occipital lobe (through burr holes) and from the scalp in 9 "normal" human subjects. He frequently observed following in the optic radiations when none could be detected in the scalp tracings.

In most work with human subjects it is feasible to obtain only recordings from the scalp. Under such conditions the brain rhythm follows the stimulus down to about 3 per second and up to about 25 per second (**145**). Evoked responses are maximal with flicker in the alpha frequency band. Following above 25–35 per second is rare (**56, 145**). Following at low frequencies is more easily evoked in infants (**124, 199**) and

children (**55**, **199**), and at high fre-
quencies in old people (**145**, **199**).

In addition to the evocation of
brain waves at the frequency of the
stimulus (fundamental frequency),
cortical potential fluctuations are
often observed at harmonic or sub-
harmonic frequencies (**17**, **56**, **145**,
199, **202**). For example, with a stimu-
lus frequency of 12 fps, evoked po-
tentials may occur at 24 per second
or at 6 per second. Third harmonics
and subharmonics are also seen.
Evoked responses do not, however,
always occur exactly at multiples or
submultiples of the flash frequency,
but may deviate by plus or minus a
few cps (**199**).

The topographic distribution of
harmonic and subharmonic responses
in the brain is often more widespread
than that of responses at the funda-
mental frequency, which are usually
maximal in the occipital-parietal re-
gion (**145**). Third harmonics are
seen significantly more often among
old people than among young adults,
second subharmonics significantly
less often.

Walter and Walter reported that
the regularity and constancy of re-
sponses tend to increase during the
first few minutes of stimulation, while
anomalous responses tend to subside,
and cite these observations as evi-
dence of long-term recruitment and
facilitation, and of extinction or
adaptation, respectively (**199**).

Sensory Effects

In addition to the primary sensa-
tion of visual flicker, a number of
illusory effects, both in the visual (**13**)
and other modalities, have been
noted. These are of considerable in-
terest, for the revelation of the mech-
anisms of such illusory effects may
yield information about the normal
functioning of the nervous system.

First the larger topic of visual illu-
sions.

Fusion. The best known illusion
is that of fusion: above a certain
flash frequency a flickering light ap-
pears subjectively as a steady light.
The lowest flash frequency at which
this effect appears is called the criti-
cal fusion frequency (cff), which
varies with a number of factors (**13**).

A point of controversy is whether
the mechanism of fusion is central or
peripheral (retinal) (**47**). Halstead
and his colleagues (**67**, **194**, **195**) sug-
gest that, since evoked rhythmic re-
sponses in the visual cortex break
down at about the cff, whereas those
of subcortical and peripheral struc-
tures follow the stimulus to higher
frequencies, the fusion mechanism is
a cortical one. Knox has reported
that the subject's attitude can affect
the cff (**109**), and that simultaneous
auditory "flicker" enhances the "pro-
nouncedness" of visual flicker (**111**);
these effects must be central. On the
other hand he was unable to demon-
strate an effect of auditory "flicker"
on the cff itself (**110**).

Brightness enhancement. At high
flash frequencies, the subjective
brightness of a repetitive light stimu-
lus is less than that of a steady light
of the same intensity, but at about 10
fps it appears brighter—the well-
known Bartley effect. This rate, 10
per second, coincides with the fre-
quency of the alpha rhythm. It is
also the frequency at which maximal
evoked rhythmic responses of the cor-
tex are obtained both with light
flashes to the retina and with direct
electrical pulses to the optic nerve
(**13**). Bartley concludes that "a ma-
jor component of the optic-cortex re-
sponse to light involves the same cor-
tical elements as the alpha activity."

Synesthesia. In addition to evoking
such previously reported visual illu-
sions as pattern, color, and move-

ment, which have been abundantly confirmed in recent EEG studies (17, 55, 145, 188, 197, 199), visual flicker stimulation sometimes evokes illusory sensations in other modalities. Kinesthetic sensations, such as swaying, swinging, spinning, and rolling occur fairly frequently during flicker stimulation (57, 145, 199), visceral sensations occasionally. Cutaneous sensations (tingling, prickling) are also reported (199). Auditory, gustatory, and olfactory sensations are rare, but do occur (145, 199). Synesthetic effects are reportedly associated with strong evoked cortical responses in the region of the appropriate sensory area (199).

Walter (199) attributes anomalous, nonvisual effects to interaction between rhythmic evoked responses and harmonically related spontaneous rhythms in other circuits at the thalamic level. Visual subjective effects he attributes to interference between evoked activity and spontaneous rhythms at the cortical and possibly also thalamic levels.

According to Walter and Walter, any of the illusory effects of flicker stimulation and the evoked cerebral responses may be accentuated or diminished by the mood or mental activity of the subject. If the subject reinforces the illusory effects from his memory or imagination, the cortical electrical responses are augmented; if he inhibits the illusory effects the electrical responses are diminished (199).

In addition to visual illusions and synesthesias, outright hallucinations, feelings of pleasantness and unpleasantness, and anxiety states may be provoked in some subjects.

Background Activity Changes

Changes in background activity of the cerebral cortex following the onset of a stimulus depend upon the activity pattern present at the time of stimulation (114), which in turn is related to the condition of the subject.

The best known of these effects is blocking of the alpha rhythm (121). The alpha rhythm, which is most prominent when the subject is relaxed and unstimulated, is said to block when it is noticeably reduced in amplitude. Although a stimulus of any modality can block the alpha rhythm—blocking by olfactory stimulation being the most recently demonstrated (4)—visual stimuli are clearly the most effective. This, together with the fact that the alpha rhythm is of highest voltage in the occipital region, has been taken to imply that the alpha rhythm is more closely related to visual processes than to other sensory processes (1).

After initially blocking at the onset of a continuous stimulus, the alpha rhythm may reappear (especially if the stimulus is monotonous or not meaningful) and then block again at the "offset" of the stimulus. These phenomena could be related to the presence of "on-off" and "off," as well as continuously firing, elements in the optic nerve. Further, the alpha rhythm may block if the subject merely imagines a stimulus, or if he concentrates on an abstract problem. Alpha blocking has thus been interpreted as related to attention to stimuli rather than to stimulation per se.

When low-voltage fast activity rather than the alpha rhythm predominates in the EEG, as when the animal is alert, the onset or offset of a stimulus is followed by no change in the background activity. On the other hand, if the animal is drowsy or in light sleep, the onset of a stimulus may be followed by the appearance of the alpha rhythm. Continuance of the stimulus (especially if it

is not of great intensity and is monotonous) may be followed by the redisappearance of the alpha rhythm and the reappearance of the patterns of drowsiness or sleep. If however the stimulus is attention-getting, and the animal is fully aroused, the low-voltage fast pattern typical of the state of alertness will replace the alpha rhythm. In deep sleep a stimulus produces no change in the EEG unless it is of extraordinary intensity.

A Visual Attention Hypothesis

Among the perennial questions in electroencephalography are those concerning the nature and functions of the "spontaneous" brain rhythms. The alpha rhythm has received most attention, and there will be occasion in this review to mention several of the hypotheses which have been advanced concerning its function.

In 1943 Adrian (1) suggested that, since the alpha rhythm appears to be more closely associated with vision than with other functions, it might be an alternative form of excitation which competes with afferent visual impulses for control of the cortex, and when it is in control, "enables us to divert our attention from visual stimuli and yet to keep the unoccupied brain from falling asleep. Auditory and tactile stimuli can do this, but vision has a far more important effect on the levels of activity of the brain. Certainly the α rhythm is the characteristic product of the brain of one who is not seeing but is still awake." The many people who do not show the alpha rhythm at all "may be people who cannot withdraw some degree of attention to the visual field as long as they are awake."

An early finding of Jasper and Cruickshank (85) appears to support Adrian's hypothesis. They observed an inverse relationship between the strength of the alpha rhythm and the strength of visual afterimages, the afterimages waxing when the rhythm waned, and vice versa.

Walsh (197) undertook an experimental test of Adrian's hypothesis. He exposed subjects, reclining in darkness and fixating a small red light, to 1-10 luminous dots presented tachistoscopically. Ability to recognize the patterns did not appear to depend on the amplitude of the alpha rhythm. He then exposed his subjects alternately to trains of clicks and flashes. The amplitude of the alpha rhythm when the subject was counting clicks did not differ significantly from the amplitude when he was counting flashes. Walsh concluded that his results "do not support the 'visual attention' theory."

An Hypothesis of the Alpha Rhythm as a Sensory Timing Mechanism

The alpha rhythm as recorded from the scalp consists of synchronized potential fluctuations in particular aggregates of cortical neurons. These potential fluctuations probably "represent synchronized oscillations in membrane potentials, possibly involving interneurons and dendrites in the cortical matrix, oscillations which would have a definite effect upon neuronal excitability, but not dependent upon neuronal discharge" (119). The validity of the excitability-cycle assumption is supported inferentially by experimental evidence of Bartley and Bishop (14), Bishop (18), and Bartley (12), and more directly by that of Chang (25–27), Gastaut et al. (53), and Morin et al. (138). It is not however certain that the cortical excitability cycles demonstrated in the last-mentioned studies are directly related to the alpha rhythm.

Assuming, for the sake of argument, that the alpha cycle actually

does represent an excitability cycle. it has been proposed as a means of coding or timing sensory impulses, "in order that our perceptual world and our reactions to it are not distorted or smeared by the more or less continuous influx of sensory stimuli" (124). The proposed mechanism is relatively simple. The probability that incoming impulses will cause a neuron to fire will vary with the phase of the excitability (alpha) cycle. Impulses arriving at synapses when the trans-synaptic neuron is in the phase of increased excitability will be more likely to fire the trans-synaptic neuron. Those arriving during the phase of lowered excitability will be less likely to do so. And when the excitability cycles of a group of neurons are synchronized, then the flow of impulses through that group will be timed by the frequency and phase of the cycle.

Jung and his colleagues (89) have conducted microelectrode studies of the discharges of individual cortical neurons. They distinguished three kinds of neuronal discharges, classified according to their relation to normal brain rhythms: (a) spike discharges entirely independent of normal cortical rhythms (such as alpha), (b) discharges associated with certain phases of the spontaneous rhythms, and (c) rapid bursts of discharges associated with the surface-positive evoked potentials in sensory areas following stimulation. The vast majority of all cortical neurons belong to class a. Neurons of class b, which behave in a way predicted by the timing hypothesis, were much less frequently found in these experiments.

A major objection to the identification of the alpha cycle as a timing mechanism is that the alpha rhythm blocks during the reception of stimuli to which the animal is paying atten-

tion. This objection is met by drawing a distinction between the alpha rhythm and alpha activity (124). The alpha-activity cycle is presumed to be a basic mechanism of the individual brain cell, but it is only when thousands of cells are acting in synchrony that there is sufficient voltage summation to produce a recordable alpha rhythm. A low-voltage fast EEG, as when the subject is alert, may represent a "fractional synchronization" in smaller aggregates of cells, but with random phase relations. If this is true, alpha activity may exist in smaller aggregates of cells in the absence of a recordable alpha rhythm in the standard EEG.

For a more detailed discussion, see Lindsley's original paper (124).

RESPONSE PROCESSES

Beta Blocking and Voluntary Movement

Jasper and Penfield (86) have observed that the beta (low-voltage 18–30 cps) activity of the precentral gyrus blocks at initiation of a voluntary movement (such as fist clenching), adapts, and reblocks at cessation of the movement. Continuous varied movements are accompanied by prolonged blocking. Imagining a movement is not followed by beta blocking, but readying-to-move is. Beta blocking may be quite circumscribed to the area of representation of the part moved.

Alpha Blocking and Motor Reaction Time

Simple motor reaction times to light have been found to vary from about 150 to 500 msec. (173). Since both the alpha blocking response and motor reaction may be initiated by the same stimulus and since their latencies are of the same order, it would seem reasonable that they are somehow related. Several studies

have had a bearing on this question (**5, 65, 102, 173, 183, 185**). The earlier studies, cited by Lindsley (**121**), indicated that latency of alpha blocking is reduced slightly by an associated motor response. However, correlations between alpha blocking times and motor reaction times, ranging from $.37 \pm .09$ (**85**) to $-.19 \pm .05$ (**102**), did not clearly indicate a significant relationship between the two variables. Further, alpha blocking times are sometimes longer and sometimes shorter than motor reaction times.

The most recent study in this area is that of Stamm (**173**). He found that mean motor reaction times decreased systematically and significantly under certain experimental conditions, but alpha blocking times (recorded concurrently) did not. The mean correlations between the two measures varied between .300 and .323 (σ's .152–.228) for 20 subjects. Under every condition there were subjects who responded at least part of the time with alpha blocking times longer than reaction times. Stamm concluded that motor reaction times and alpha blocking times are measures of essentially independent systems.

Alpha Activity as a Response Timing Mechanism

The hypothesis that alpha activity may serve as a means of timing sensory impulses entering the brain has already been presented. It has also been proposed that it may serve as a means of timing motor impulses leaving the brain and that these functions may be coordinate.

Kibbler and his co-workers (**97, 98**) concurrently recorded the EEG and various voluntary muscular responses to auditory stimuli. Plotting responses against phase of the alpha rhythm, they found that the proba-

bility of a response was not randomly distributed in time, but rather showed peaks and troughs recurring about 10 times a second in accurate phase relationship with the alpha rhythm. Bates (**15**) similarly found a significant tendency for voluntary movements to be initiated always at the same point in the phase of the alpha wave.

Meister (**135**) concurrently recorded EEG and saccadic eye movements in four subjects. Although readable tracings were difficult to obtain because of alpha blockade by visual stimulation, his data also indicated a significant relationship bebetween the inception of eye movements and the phase of occipital-parietal alpha waves.

The results of these studies support the hypothesis that the alpha rhythm (or alpha activity) may be a means of timing the outflow of motor impulses from the brain.

PERCEPTION

Devising theoretical models of physical processes corresponding to mental processes is useful to the psychophysiologist only insofar as such models predict how the brain itself actually operates, or give rise to constructive suggestions for future work. MacKay has offered an excellent discussion of the usefulness of brain-machine analogies (**132**).

Several theoretical neural models have been devised which offer explanations of perceptual phenomena. Only those which involve the alpha rhythm will be discussed here.

A Scanning Hypothesis

Grey Walter has postulated the need for a mechanism in the space receptor areas of the brain, "whereby the sensory field could be scanned continuously in such a way that the detailed bits of information they con-

tain could be conveyed to a central assembly by only a few channels, there to be related one with another for appropriate action to be taken" (**202**). One characteristic of such a scanning mechanism would be that "on the instant that a signal appeared on its beat it would be halted, and its position at the check would convey to all other regions the relative position of the detail of sensation. When a complex pattern appeared, the succession of runs and checks would repeatedly convert a special pattern which was constant during the time of a single sweep . . . into a series of signals on a base of time, so that all the information contained in a single parameter of sense can be conveyed on a single channel in a code of unit pulses."

The price of such parsimony is that reception speed is limited by the sweep frequency; changes in space which occur more frequently or movements which cover a great distance in the time of the sweep will result in smearing of the pattern. In addition "intermittent signals which are shorter than the duration of the sweep but recur at about its frequency will give the illusion of movement . . . . (A)nd also, if action is to be taken on the basis of information received from the field scanner . . . the outflow of centrifugal messages must be to some extent regulated by and synchronized with the time base on which the centripetal ones are coded."

Walter goes on to propose that the alpha rhythm constitutes such a scanning mechanism. It is immediately apparent that this hypothesis accounts for some of the observed phenomena already described in this paper: alpha blocking at the onset of stimulation, the movement illusion during flicker stimulation, and the synchrony of the alpha rhythm with the initiation of voluntary movements. The hypothesis can also account for the upper time limit for perception of a pattern (1/10 second) and for the phenomena of apparent movement. Walter has reported, as would be expected from the hypothesis, that in persons who employ "vivid and plastic visual images" for mental tasks the alpha rhythm is discontinuous and frequency analyses of the alpha rhythm are complex and variable (**203**).

Pitts and McCulloch independently conceived of a scanning-mechanism role for the alpha rhythm (**155**). They showed that a circuit resembling a nerve net could be drawn, which could extract from a pattern of stimuli certain invariant characteristics (for example, in vision shape regardless of size or in audition chordal structure regardless of pitch), "if it were rhythmically excited to 'scan through' the group of transformations with respect to which that characteristic was invariant." McCulloch and Pitts's nerve-net models[a] were arranged in layers (as in the cerebral cortex) and their hypothesis required that a rhythmic scanning sweep step up and down through the layers. They felt that the alpha rhythm satisfied the requirement. Their over-all scanning hypothesis is in a number of respects similar to Walter's, and McCulloch cites many of the same data in its support.

Walsh (**197**), O'Hare (**147**), and MacKay (**131**) have experimentally tested deductions from the scanning hypothesis. Walsh's deductions and results are as follows. (*a*) Reaction time should vary with the amplitude or the phase of the alpha rhythm at

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[a] For details and related speculations see the original paper (**155**) and McCulloch's Hixon Symposium paper and the discussion which followed it (**130**).

the moment of stimulation; it did not. (*b*) Visual threshold should also so vary; it did not. (*c*) Reaction time scatter should not exceed 0.1 second; it did. (*d*) The distribution of reaction times should be rectangular; it was positively skewed.

O'Hare repeated part *a* of Walsh's study using a larger number of subjects and measuring both visual and auditory reaction times. He too obtained negative results.

MacKay argued that "if size-invariance were in fact secured by a rhythmic transformation of the size of the mapped visual image in the occipital cortex at the alpha-frequency, then it would be not unreasonable to look for some kind of stroboscopic effect if the subject were presented with a visual image that was itself fluctuating in size at or about the same rate." He tested this deduction using a specially designed "pattern-generator." The predicted results were not obtained.

MacKay concluded that these failures do not disprove the scanning hypothesis, but they "appear to circumscribe the role attributable to the alpha rhythm in such speculations."

A Movement-Perception Hypothesis

When one voluntarily scans a visual field, the objects in that field do not appear blurred or smeared but remain clear and distinct. The same is true of an object moving across the visual field of a stationary eye. On the other hand, when the eye moves involuntarily, as when the eyeball is pushed with the finger or during nystagmoid movements of vestibular origin, the visual field appears to move and objects appear blurred.

Because he finds it difficult to explain these phenomena in terms of a visual system in which data (impulses) are transmitted continuously

from the retina to the visual areas of the cortex, Meister (135) has proposed that retinocortical transmission is not continuous, but rather, intermittent; that there exists a "neuronic shutter mechanism" which allows visual data to reach the cortex in "discrete units." The basic mechanism is that of summation in which the nerve elements responsible for the alpha rhythm summate with retinocortical impulses at a synaptic transmission point. The locus of such a mechanism may be either subcortical (geniculate?) or cortical, or perhaps both.

Meister's formulation requires that voluntary and saccadic eye movements be so synchronized with the shutter mechanism that retinocortical transmission occurs only when the eye is stationary. (His study of alpha phase at the initiation of saccadic eye movements, already cited, was done in this connection.)

Meister draws an analogy between his neuronic shutter mechanism and a motion picture camera which moves when the lens shutter is closed and is stationary when the shutter is opened. Thus movements of the visual field and smearing are avoided. He also shows how the hypothesized mechanism can explain the phenomena of apparent movement, gamma movement, the spoke illusion, and Charpentier's bands, and how disorders of the system may account for such pathological phenomena as the quick motion illusion and monocular diplopia and polyopia.

The similarity of this "shutter" hypothesis to the timing and scanning hypotheses is apparent. It is interesting to note that these similar hypotheses were arrived at independently and that many of the same data from the literature have been cited in support of each of them. Lindsley (124) has cited Meister's

hypothesis as an example of the application of the broader timing hypothesis.

COMPLEX PROCESSES

A *Diffuse Thalamic Projection System*

Recent work has revealed the existence of a diffuse thalamo-cortical system, separate from the better known specific projection systems, and has determined some of its relationships with other parts of the brain.

In the early 1940's Morison and Dempsey (**41, 42, 139**) reported recording electrical waves from widespread areas of the cortex during low-frequency repetitive electrical stimulation in the vicinity of the internal medullary lamina of the thalamus These responses exhibited recruitment, that is, they increased in amplitude with the first few stimuli. These recruiting potentials were similar to spontaneous 8–12 cps potentials in a number of respects, and Morison and Dempsey suggested that they are identical. Since World War II the work of Morison and Dempsey has been extended, principally in the laboratories of Jasper (**2, 68, 69, 79, 82-84**) and Magoun (**174, 177, 192, 193**).

Anatomy. For details of the thalamic structures involved and their connections, see the papers just cited. Jasper has called the elements of these structures involved in recruiting responses the "thalamic reticular system."

Physiology. Single electric pulses delivered to the thalamic reticular system evoke rhythmic bursts of 8–12 per second waves in the cortex. Repetitive stimuli at 8–12 per second are accompanied by augmentation of the natural cortical rhythm. Stimuli at 4-5 per second tend to evoke double responses (at 8–10 per second) and slower stimuli even triple re-

sponses. Stimuli at 15–20 per second tend to evoke responses only at every other pulse. Stimuli at 30–50 per second suppress the recruiting response (**82**).[4] Simultaneous stimulation of the BSRF and the thalamic reticular system results in suppression of the recruiting response (**69**).

If the thalamic reticular system is stimulated strongly, it tends to function as a unit, evoking recruiting responses in all or most of the cortex (**177**). If, however, just-adequate stimuli are delivered to discrete, portions of the system with small bipolar electrodes, more discrete projection to the cortex can be demonstrated (**69, 82**).

All recruiting phases of the recruiting response are of local origin, that is, recruitment occurs in the thalamus independently of the cortex and vice versa (**69, 192**). Each local response is believed to represent an oscillatory phenomenon developing along closed chains of neurons. The time relations of the responses from deep structures and those in the cortex of the intact brain suggest however that the processes are closely related. Verzeano *et al.* (**192**) have hypothesized a complex of reverberating circuits, consisting of intracortical and intrathalamic reverberating circuits plus thalamocortical and corticothalamic connections between them, to account for the conditions observed.

Functions of the diffuse thalamic projection system. It seems to be agreed among those who have been

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[4] Jung and Riechert have recently demonstrated recruitment phenomena following stimulation in the medial and intralaminar thalamic nuclei in man, but they were unable to demonstrate the suppression of cortical rhythms by high-frequency (30–100 per second) stimulation (**90**). Hassler and Riechert however were able to produce behavioral arousal with high-frequency stimulation of the intralaminar nuclei or centre median (**71**).

investigating this system (69, 82, 83, 84, 177) that, because of its structure and its connections and functional relationships with the cortex and deep brain structures, it is ideally suited to function as a central integrative mechanism participating in such functions as learning and thinking. Jasper and Ajmone-Marsan (84) suggest that it can regulate the arrival and the elaboration of impulses within the sensory receiving area of the cortex, and since the action of the thalamic portion of the system can be limited to one specific system, a mechanism for the central control of attentive processes is suggested. This is a general form of Adrian's visual attention hypothesis (above) but one which does not suffer from Walsh's negative experimental results. Hunter and Jasper (79, 82) and Penfield (152) believe that the diffuse thalamic system is also involved in the genesis of some kinds of epileptic seizures.

Conditioning, Learning, and Memory

Conditioning of the Alpha Blocking Response

It has been repeatedly demonstrated that the alpha blocking response can be conditioned (80, 87, 106, 116, 128, 140, 143, 144, 184). The usual procedure is to use a weak sound stimulus as the conditioned stimulus (CS), one which by itself usually does not elicit alpha blocking. A light is the unconditioned stimulus (UCS). After only a few pairings of CS and UCS, the alpha rhythm will block to the CS alone, constituting the conditioned response (CR). The criterion of conditioning is usually 2–5 CR's without UCS. Extinction is relatively rapid (87, 128, 184).

Jasper and Shagass (87) established CR's in 20 trials with 10 seconds delay between CS and UCS; delayed CR's were always anticipatory.

The longer the delay the more trials were needed to condition. Delayed conditioning was also reported by Morrell and Ross (140) and Iwama (80). Trace conditioning has been established with 4 seconds delay (106) and 9–10 seconds delay (87). Differential conditioning (80, 87, 140, 144), and cyclic and backward conditioning (87) have also been demonstrated, but backward conditioning required 100 trials to a criterion of 2 consecutive CR's without UCS.

Shagass (165) conditioned the alpha blocking response to a voluntary CS (fist clenching) in 7 of 8 subjects. The number of trials to a criterion of 5 CR's without UCS varied from 8 to 138. Shagass and Johnson (167) showed that the acquisition curve for such a CR, using a procedure in which half of the trials were reinforced, was an accelerated one, nearly the mirror image of the extinction curve. Acquisition and extinction curves were similar to those obtained when peripheral responses are conditioned.

Laufberger (116) reported conditioning the alpha blocking response using imaginary CS and UCS in about 100 trials. The CS consisted of thinking of the nonsense syllable "ki," the UCS of thinking of a light. Extinction was not mentioned.

Motokawa and Huzimori (144) recorded the EEG during the acquisition of a conditioned galvanic skin response (GSR), using a bell as CS and faradic shock as UCS. They distinguished 3 EEG responses which they termed "excitation potentials" ("Ep"): (a) alpha blocking, (b) beta augmentation, and (c) irregular baseline deflections. The last may have been artifacts, but the authors believed not. The first two are identifible with Magoun's activation pattern.

Motokawa and Huzimori observed

that the "Ep" response developed during the acquisition of the conditioned GSR, occuring before the GSR itself; it was more easily established as a CR and resisted extinction longer. During extinction the basic alpha rhythm appeared stronger than before or during conditioning. Spontaneous disinhibition during extinction was associated with an excitation pattern in the EEG. During the delay period in delayed conditioning the alpha rhythm was unusually strong; the "Ep" response occurred about 2 seconds before the peripheral CR, and was protracted through the period of the peripheral response. The authors proposed that the peripheral CR is secondary to a central CR of which the excitatory response of the cerebral cortex is a component, while the presence of a strong alpha rhythm is indicative on the other hand of a cortical inhibitory state.

Further data pertaining to ·these propositions have been reported. Motokawa (**143**) found that it is easier to establish differential conditioning of the "Ep" response than of peripheral responses. Iwama (**80**), working in Motokawa's laboratory, established a delayed alpha blocking CR using a metronome as CS, with a delay period of 20 seconds. At the onset of the metronome the amplitude of the alpha rhythm increased and at about the 20th second was replaced by beta activity. He interpreted the initial alpha augmentation as an indication of internal inhibition. Extraneous stimuli presented during the delay period resulted in a long-lasting train of beta waves (disinhibition). After differentiation or extinction were established it was found that alpha waves were well-developed in the whole extent of the oscillogram (differential or extinctive inhibition).

Morrell and Ross (**140**) condi-

tioned the alpha blocking response to a buzzer (CS). The UCS was a flickering light. Reaction times to the light stimulus were obtained concurrently with the EEG record, thereby providing a measurement of cortical conduction time each time the CR was reinforced. When the light stimulus was presented after the CR was extinguished, the reaction time was found to be prolonged (up to 800 msec.; pre-extinction time 200–300 msec.). Differential inhibition (sounding a whistle, the CR to which has been extinguished, simultaneously with the buzzer) and delayed inhibition (conditioning alpha to block after a delay of 6 seconds and then presenting the light in the interval) also lengthened the reaction time. The authors assumed that retinocortical time and corticomotor time are constant and so the increase in reaction time is due to an increase in intracortical transmission time. Therefore they interpret their results as confirmation of the Pavlovian hypothesis that extinction of the CR gives rise to a process causing an increase in cortical transmission time.

Alpha Blocking and Stimulus Trace

Knott *et al.* (**108**) sought to determine if the blocking of the alpha rhythm during and following a brief stimulus might serve as a measure of central neural activity which could be correlated with stimulus trace as postulated by Hull. They measured the total peak-trough amplitude of alpha waves in mm. for a number of short intervals during the presentation of a 3-second auditory stimulus and also following a 0.2-second stimulus. The total amplitude obtained was divided by the mean prestimulus amplitude, yielding an amplitude ratio. The shape and duration of amplitude ratio curves (plotted against time), when compared with plots obtained in earlier investiga-

tions of the CR as a function of the CS-UCS interval, suggested that correlates of Hull's stimulus trace and perseverative stimulus trace are provided.

EEG Changes During Serial Learning

Obrist (**146**) tested several corollaries of a hypothesis that rate of learning is a function of the excitatory effect of the stimulus upon the organism. Fifteen college students learned a list of 16 equated nonsense syllables to a criterion of one perfect recitation. GSR and EEG were recorded during the learning trials and during a 3-minute nonlearning control period. Central excitatory effect was measured for each syllable and each trial in terms of (a) summed GSR in log-conductance units and (b) summed alpha voltage, on the basis of previous experimental evidence indicating that degree of arousal is directly proportional to GSR and inversely proportional to alpha amplitude.

Magnitude of GSR was positively related to (a) learning as contrasted with nonlearning, (b) individual rate of learning, (c) individual rate of learning of single syllables, and (d) group rate of learning. A typical bow-shaped serial position curve was obtained for magnitude of GSR when group means were plotted.

Alpha voltage was significantly less (7–14%) during learning than during nonlearning, and a positive correlation of .60 was obtained between subjects' alpha frequencies and their rates of learning, but there were no significant correlations between alpha voltage and (a) rate of learning, (b) number of correct anticipations, and (c) magnitude of GSR.

Obrist concluded that his GSR findings lend support to the hypothesis that learning is associated with a high degree of arousal or attention,

and that attention is maximal for a given group of syllables during the time when the greatest amount of learning takes place. "Contrary to most present-day theories employing concepts of inhibition, the GSR results suggest an explanation of serial position effects in terms of a factor of excitation. . . ." He also feels that frequency changes in EEG may be meaningfully related to the learning process.

A Neural Mechanism for Memory

Some epileptic patients experience minor seizures, or auras preceding major convulsive seizures, which consist of memory illusions (*déjà vu, jamais vu*) and/or memory hallucinations (dream-like auditory or visual experiences derived from the patient's earlier life). These illusions and hallucinations differ from those reported by psychotics in that the patient is quite aware of their unreality, however vivid they may be. Epileptogenic foci in these cases are revealed by the EEG to lie in the temporal region.

Penfield and his colleagues have, over a period of more than 20 years, operated upon hundreds of patients with such psychic seizures and other forms of focal epilepsy (**153, 154**). During operation they have determined the effects of direct electrical stimulation at thousands of cortical and subcortical points. The position of the stimulated points is recorded by photography, the electrical responses of the brain by electrography with the recording electrodes on the cortex or in the brain substance, and the motor, sensory, and psychic effects of the stimulation are recorded by sound recording of the patient's and surgeon's comments. The operations are performed under local anesthesia.

In a number of cases memory

illusions and hallucinations have been elicited by stimulation of the temporal lobe or the adjacent posterior-parietal cortex. They have never been elicited by stimulation elsewhere. Generally auditory hallucinations are evoked by stimulation of the anterior part of the temporal lobe and visual from the posterior, but this is not invariable. Repeated stimulation of a given point will often elicit the effect over and over again, but sometimes hallucinations will change or a different memory will be evoked. Such instability of stimulation effects is not peculiar to these phenomena, having been observed elsewhere in the brain, even in the primary motor cortex.

Penfield has proposed (**151**) that the recording and retention of information are primarily functions of the "memory cortex" (the temporal cortex of both hemispheres, excluding Heschl's gyrus and some portions of the inferior surface) in conjunction with a central integrating system. The integrating role is attributed to what he and Jasper have called the "centrencephalic system," which is defined as "that neuronal system of the higher brain stem which has been in the past, or may be subsequently shown to have equal functional relationship to the two cerebral hemispheres." Under this definition are included the BSRF and the thalamic reticular system, the latter of which at least has connections with all of the sensory and language areas of the cortex. Cortical sensory areas are considered as way-stations between the periphery and the central integrating system.

According to Penfield's hypothesis, summation and fusion of all types of sensations are achieved in the centrencephalic system, and from there they are projected to the temporal cortex of both hemispheres, where they lie dormant. Recording is assumed to occur by synaptic facilitation produced by the passage of impulses. Voluntary recall is effected by impulses from the centrencephalic system to both temporal cortices in pathways similar to those originally followed by the impulses that laid down the memory pattern. These impulses evoke the original pattern of cortical discharges, which runs off as a time series. Specific patterns may also be evoked by epileptic discharges in, or direct electrical stimulation of, the temporal regions of the brain.

The following objections to this hypothesis were made by Lashley during the discussion of Penfield's paper (**151**). (*a*) Bilateral removal of the temporal lobes in animals does not completely abolish memories, since they can be recovered spontaneously, and therefore memory traces cannot be stored exclusively in the temporal lobes. (*b*) The small numbers of neurons in the thalamic nuclei and the centrencephalic system and the paucity of interneurons make it difficult to attribute the mediation of complex functions to those structures.

To these arguments Penfield replied: (*a*) In higher mammals equipotentiality is less striking than in lower. (*b*) If there is no local patterning, how can memories be evoked by local stimulation? (*c*) Since the centrencephalic system is not exclusive, but functions with other areas, including the entire cortex, its paucity of neurons is not a valid objection.

In a later paper (**152**) Penfield suggests that the centrencephalic system may be divided into an A-mechanism and a B-mechanism. The function of the A-mechanism is to record conscious perceptions in persisting neuron patterns, and the B-

mechanism functions in recollection of past experiences and in the integration of the sensory and motor systems. It has further been suggested (153) that the temporal-parietal cortex in conjunction with the pulvinar-lateralis-posterior complex of the thalamus plays a major role in the initial integration and elaboration of incoming sensory patterns.

Penfield's hypothesis clearly does not provide a complete account of the neural events underlying learning and memory. At present the hypothesis rests almost entirely upon one fact, that of the temporal localization of memory engrams in epileptics. That the centrencephalic system is an integrating system, however logical it may seem, is at present only an assumption. But the hypothesis is intriguing and should give rise to additional research.

Intellectual Processes

Intelligence

Studies of relationships between EEG variables and intelligence have been relatively few. Lindsley (121) reviewed the literature up to 1944 and concluded that "it appears doubtful that there is any very high degree of relationship between intelligence as measured by tests and the EEG." Ostow drew a similar conclusion in 1950 (148).

Only three references to the relation between EEG and intelligence have appeared since Ostow's review. Kreezer and Smith (112) correlated Stanford-Binet MA with various properties of the alpha rhythm in a group of mental defectives. The correlation between MA and alpha frequency with CA partialed out was .22 (not significant); correlations between alpha amplitude and MA

and alpha index and MA were negligible.

Rey, in a study cited by Hill (78), found that patients with paroxysmal EEG abnormalities differed from those with nonparoxysmal abnormalities at the 1 per cent level of confidence with respect to having verbal scores lower than nonverbal scores, using the Mill Hill Vocabulary test and the Ravens Progressive Matrices. The tests did not distinguish between persons with nonparoxysmal abnormalities and those with normal EEG's.

Walter (203) reports a measure of the "versatility" of the brain which he believes is related to intelligence. "Versatility" is defined in terms of the variability of the EEG frequency spectrum (obtained by automatic frequency analysis) from one 10-second epoch to another. The less intelligent subject is reported to show a lack of versatility in this sense, that is, his frequency spectrum is much the same from one short epoch to the next. The very intelligent subject is reported to display great variability.

The weight of evidence indicates that the alpha rhythm is unrelated to test intelligence. The findings of Rey and of Walter await confirmation.

EEG During Mental Effort

Evidence is found in the early EEG literature (121) that intellectual activity tends to be accompanied by alpha blocking and an increase in beta activity. Similar observations have been reported a number of times (66, 101, 120, 134, 172, 186).

Knott (101) reported that the mean brain-wave frequency increases and the distribution becomes more skewed to the high end during periods of silent and oral reading as compared with periods of rest. No dif-

ference was observed between the effects of oral and silent reading. Hadley (66) reported a related increase in mean brain-wave frequency and heart rate during the solution of mental arithmetic problems. There was no relationship between EEG or heart rate changes and the difficulty of problems; muscle potential activity, on the other hand, did vary with problem difficulty.

Liberson (120) recorded the EEG during a word association test. He reported a relationship between alpha blocking time and reaction time, but neither complete data nor coefficients of correlation were given. He found no differences in alpha blocking time for emotional as compared with nonemotional stimulus words. Martinson (134) found no relationship between alpha frequency, alpha index, and mental blocking.

In contrast with the findings of others, Toman (180) found no marked or consistent EEG changes during the solution of mental arithmetic problems in 64 medical students. He reported that initial alpha blocking did occur, but considered it as a response to the stimulus only and not related to the problem-solving process. He noted that there were marked individual differences in EEG pattern changes. It is difficult to reconcile Toman's report with the demonstrations by Knott and Hadley of significant changes in the EEG frequency spectrum, especially since Hadley's subjects were performing the same kind of operations as Toman's.

Kennedy and co-workers (92, 93, 94, 95) have reported recording an independent brain rhythm associated with intellectual processes. With recording electrodes on the temples, an 8–12 cps rhythm of around 20μV amplitude occurring in spindle-shaped bursts is recorded. These have been named kappa waves. Kappa activity is markedly accentuated during reading, mental arithmetic, difficult discriminations, learning and memory tasks, and problem solving. It is most pronounced during recall of previously learned material.

Kappa waves could be recorded in only about half of the subjects tested. Changing of electrode placements and retesting did not result in recording kappa waves in subjects who had yielded negative results initially.

Despite similarity of frequency and amplitude, kappa waves are apparently not a form of alpha activity, since they do not react to stimuli as the alpha rhythm does, and they wax during intellectual activity whereas the alpha rhythm wanes. Kennedy *et al.* have presented evidence indicating that kappa waves are of cerebral origin (arising in the temporal lobe, they believe).

Bickford (personal communication), however, believes that eye-movement artifact has not been satisfactorily excluded in the genesis of these waves. The observation by Teitelbaum (178) of spontaneous rhythmic ocular movements, predominantly in the horizontal plane, during mental concentration is perhaps significant in this regard. Unfortunately Teitelbaum did not measure the frequency of the movements.

Kennedy's results have been neither confirmed nor refuted by other workers. The last publication concerned with kappa waves from any laboratory appeared in 1949.

Alpha Responsiveness and Mental Imagery

During the last decade studies of the physiological concomitants of mental imagery have been conducted by workers at the Burden Neurological Institute (62, 168, 169). The

simplest exposition of this work is to be found in Walter's book *The Living Brain* (203).

Briefly, modes of imagery have been related to two physiological variables, alpha-type and respiration. The type of imagery which a subject is disposed to use is determined by questioning him about his methods of undertaking certain assigned mental tasks. Two modes of imaging are distinguished, the visual and the verbal-kinesthetic. Other modes, it is stated, are unusual (168). Generally subjects can be classified as (*a*) either visualists or verbalists who find it difficult to shift from one mode to the other, and (*b*) those who use predominantly one mode, but can shift without great difficulty. The latter are in the majority.

Three alpha-types are distinguished: (*a*) the P (persistent) type, showing a strong alpha rhythm which persists in spite of stimulation or intellectual activity, (*b*) the R (responsive) type, showing a good alpha rhythm during relaxation which responds readily (blocks) upon stimulation and during intellectual activity, and (*c*) the M (minus) type, showing no alpha rhythm or only a few low voltage alpha waves even during relaxation. Walter points out that most studies in the literature relating alpha activity to various psychological processes have involved only subjects showing strong alpha rhythms in order to facilitate frequency and amplitude measurements. This has resulted in the almost complete neglect of the M group.

Two types of respiratory activity are distinguished, regular and irregular.

The findings of Walter and his colleagues have been essentially that subjects of the P alpha-type are predominantly verbalists; during intellectual activity their breathing is irregular, apparently due to activity of the vocal apparatus. Subjects of the M type are predominantly visualists, and during intellectual activity their breathing is regular. Subjects of the R type are either visualists or verbalists, but can usually shift to the other mode without great difficulty. The M and P types are more successful in executing motor tasks when depending only on stereognosis for sensory cues (169); such skill is attributed to their consistent use of one type of imagery, which type being apparently of little importance.

These findings recall, and indeed appear to support, Adrian's visual attention hypothesis (1; see above). They also raise a number of questions (203). For example, what is the origin of the differences observed? (There is some evidence that alpha-rhythm characteristics are hereditary.) At what stage of development do differences in alpha responsiveness become apparent? (Strong alpha rhythm is commoner among children than among adults, M-type EEG's less so.) To what extent do the ways of thinking imposed by these modes of imaging affect or determine personality?

Hypnosis

EEG Changes During the Induction of Hypnosis

As the subject passes from the normal into the hypnotic state no changes can be detected in the EEG pattern by inspection (10, 43, 50, 72, 121, 129, 170). This evidence would indicate that the hypnotic state does not differ physiologically from normal wakefulness. Physiological sleep, however, can be induced by hypnotic suggestion, or may occur spontaneously, under appropriate conditions (9, 10, 72).

Darrow *et al.* (35) have reported

that measurement of phase relationships between tracings derived from the motor and occipital areas of the brain under hypnosis (as compared with normal wakefulness) show a small, but statistically significant, increase of average in-phase correspondence during expiration. A comparison of frontal and motor tracings shows a small, but statistically significant, increase in "parallelism or synchrony." Similar changes occur during the onset of normal sleep in some subjects (36). They concluded that "hypnosis is not differentiated by this criterion from 'hypnoidal' states preceding and following sleep." These observations have not been repeated by other workers.

Gerebtzoff (59) induced cataplectic states in rabbits by "fascination," ocular fixation, or turning. The spontaneous electrical activity of the cortex was replaced by a slow-wave pattern indistinguishable from that of normal sleep. Cortical responses to sensory stimuli were markedly weakened, and sometimes only the after-discharge was seen. By this evidence such states in animals do not correspond to hypnotic states in man.

EEG Changes Under Hypnotic Suggestion

Three out of four studies have shown that, when hypnotic suggestion is made that a visual stimulus is present, the alpha rhythm blocks even though no objective stimulus is presented (9, 121). Lundholm and Löwenbach (129) failed to elicit such a response. They also obtained negative results with auditory stimuli. They were unable to explain the discrepancy between their results and those of earlier studies.

On the other hand, suggestion of blindness does not prevent alpha blocking when a light stimulus is actually presented, even though subjectively the light is not seen (9, 50, 121, 129). Loomis *et al.* (128), however, reported that suggestion of blindness to a hypnotized subject with eyes open in a lighted room was followed by the appearance of the previously absent alpha rhythm. They also reported that suggestion of anesthesia did not prevent alpha blocking following a pin prick, even though the prick was not felt.

Barker and Burgwin (9, 10) reported successful induction of sleep patterns in the EEG by hypnotic suggestion, if the subject were properly prepared. Subjects instructed to remember events during hypnosis did not remember events occurring while the EEG showed sleep patterns as well as events occurring while the EEG showed wakefulness patterns (10).

By hypnotic suggestion to relax, Ford and Yeager (50) reported the induction of "good" alpha patterns in several subjects with anxiety states, whose previous EEG's showed little or no alpha rhythm. Relaxation suggestions were not followed by EEG changes in subjects whose EEG's naturally showed "good" alpha rhythm. These investigators also regressed two subjects, who had undergone craniotomies, to their preoperative periods. Their EEG's retained postoperative patterns.

Emotion

EEG Patterns Associated with Emotional Reactions

In 1948 Lindsley (122) reviewed the literature pertaining to emotion and the EEG. He concluded that "under conditions involving some degree of emotional arousal, as in apprehension, unexpected sensory stimulation, and anxiety states, two principal kinds of changes are re-

flected in the EEG: (a) a reduction or suppression of alpha rhythm, and (b) an increase in the amount of beta-like fast activity." These changes will by now be recognized as constituting part of the activation response of Magoun, Lindsley, et al.

In recent years several investigators have studied emotional states with the newer techniques of photic flicker stimulation and automatic frequency analysis. Ulett et al. (189) correlated the EEG's of 191 subjects, recorded under standard and photic stimulation conditions, with an 8-point rating scale for anxiety proneness derived from psychiatric interviews and psychological testing. Significant correlations were found between anxiety-proneness ratings and (a) amount of subjective dysphoria during photic stimulation, (b) amount of harmonic EEG response in the 20–30 cps range to flicker frequencies $\frac{1}{2}$ and $\frac{1}{4}$ that rate, (c) displacement of the centroid of driving from the normal range, and (d) percentage of abnormal and low alpha records. A check list of EEG anxiety-indicators correlated with the criterion ratings .48. Under experimental stress (threat of electric shock) the occipital driving response acted the same way as does the alpha rhythm, that is, its "power" was reduced. However, there was no difference between subjects with anxiety and those without, with respect to the magnitude of the effect.

Ulett and Gleser (188) developed from their data 3 empirical scales designed to differentiate the anxiety-prone. The 3 scales were based on the basic EEG record, EEG response to photic stimulation, and subjective sensations induced by flicker. Each of the scales identified more anxiety-prone than nonanxiety-prone subjects from the original experimental population. Used in com-

bination, they identified 59.4 per cent of the anxiety-prone normal subjects and 65.5 per cent of the anxious patients, with only 11 per cent "false positives" (nonanxiety-prone subjects). These scales were cross validated on an independent sample of 110 (190). The scales based on basic EEG record and subjective sensations induced by flicker held up, that based on EEG response to flicker did not.

Knott and Correll (104) reported a number of driving effects at fundamentals and harmonics of stimulus-frequencies in 14 stutterers. Although these differed significantly (1% level) from effects exhibited by controls, they were not striking and did not appear to be of the type described by Ulett in the anxiety-prone. In contrast with Ulett's findings suggesting lower "alpha power" in the anxiety-prone than in the non-anxiety-prone, it was found that normal speakers showed slightly less relative alpha-voltage than the stutterers.

Tracings derived from nasopharyngeal electrodes, presumably picking up activity of hypothalamic origin, show changes similar to those derived from cortical (scalp) electrodes (122). It is, however, quite likely that nasopharyngeal tracings actually represent the activity of the mesial-inferior temporal cortex more than that of the brain stem.

Slow waves have several times been reported to be associated with emotional states. Lindsley (122) feels that those reported in the early literature were probably artifactual or associated with skin electrical changes of autonomic origin.

More recently Walter has reported (202) that emotional disturbances arising during flicker stimulation experiments are usually associated with brain waves in the

temporal region of the brain at sub-harmonics of the stimulus frequency and usually in the theta (4–7 cps) frequency band (cf. the findings of Ulett *et al.*). If, Walter reports, the subject "abandoned himself to the emotional tide," or if further emotional aggravation were added, the temporal component was accentuated. But the response was not entirely under the control of the subject or the experimenter. Spontaneous fluctuations in mood influenced the response in the same direction as short-term changes in attitude.

An Activation Theory of Emotion

Lindsley concluded his 1948 paper with some brief speculations about possible cortical-brain-stem mechanisms for the reflection of emotional changes in the EEG. In his chapter on emotion in Stevens' handbook (123) he expanded these into an "activation theory of emotion," based on the following: (*a*) the observation that the EEG in emotion presents an activation pattern, (*b*) the known physiological mechanisms of the activation response, and (*c*) the assumption that the "the mechanism of the basal diencephalon and lower brain stem reticular formation, which discharges motor outflows and causes the objective features of emotional expression is either identical with or overlaps the EEG activating mechanism . . . which arouses the cortex." After outlining the experimental evidence supporting these basic points, he presents a schema to account for varieties of emotional response and experience in terms of a hierarchy of neural mechanisms, involving progressively higher levels of the neuraxis. The possible roles of the reticular structures and the limbic lobes (6, 150) are emphasized.

Mechanisms for "Functional" Effects of Emotion on the Brain

Darrow (32, 33) has emphasized the effects not only of neural, but also of hydrostatic, chemical, autonomic, and humoral mechanisms during emotional states. The balance of such influences, he feels, determines cortical reactivity. In a number of experiments using elaborate techniques for recording EEG and peripheral autonomic activity, he has demonstrated interaction of cortical and autonomic activity. He hypothesizes that in moderation the various mechanisms for homeostatic regulation of the brain "serve to regulate and to terminate cortical excitation and to prevent self-perpetuating, circular, perseverating, and ruminative activities within the feltwork of the cortex," but that either under- or overactivity of these mechanisms may "embarrass" cortical function. Various physiological conditions may increase susceptibility of the cortex to subcortical influences, and in such cases changes in autonomic activity incidental to emotional disturbance become crucial, sometimes resulting in what he has called "relative functional decortication." Darrow suggests that the treatment of the latter conditions, which are identifiable by his testing techniques, is to improve cortical function, so that the cortex may be less readily influenced by subcortical processes. In this he is opposed to the practice of using sedatives routinely in cases of excessive anxiety or tension. It is certainly true that in many such cases subhypnotic doses of sedatives increase, rather than decrease, the patient's discomfort. This is especially true with many hyperactive children, who may become more agitated under mild doses of sedatives.

In a recent paper (34) Darrow reports data which appear to indicate that in brain-injured or emotionally unstable individuals stimulation tends to increase slow alpha-like or theta activity in the precentral parts of the brain, which he refers to as increased anterior dominance. He reports that this effect can be conditioned as can the more frequently reported alpha-blocking effect. In more stable nervous systems, "activation" and postcentral alpha "facilitation" prevail. Anterior dominance he interprets as reflecting an excitatory condition, and posterior dominance, regulation. He concludes that his evidence seems to indicate that the ability to build up post-central alpha dominance in defense against repeated and expected disturbing conditioned and unconditioned stimuli is a characteristic of 'normal' nervous systems."

Possibly because of the uniqueness and complexity of Darrow's method, his observations have not yet been repeated by other workers. Bonvallet *et al.* (19), however, have reported the results of simultaneously recording EEG and blood pressure in dogs and cats. They found that, in the absence of stimulation, spontaneous fluctuations of EEG activity and sympathetic tone are parallel, activation of the cortex corresponding to increase in blood pressure. Nociceptive stimuli produce marked increase in sympathetic tone accompanied by cortical activation, the duration of cortical activation being directly related to the intensity of sympathetic tone. They also demonstrated that peripheral sympathetic discharge does not bring about cortical activation directly, but rather by action on the pontomesencephalic structures. Small doses of sedatives suppress these effects.

Emotional Aggravation of Epileptic Processes

It is well-known that some epileptics will have seizures more frequently under emotionally disturbing conditions than under emotionally stabilizing conditions. Several workers have published data indicating an increase in EEG pathosis in epileptics during periods of emotional disturbance.

Barker and his colleagues have reported a number of cases (7, 8, 11). In some epileptics startle, and other disturbing stimuli, as well as verbal stimuli aimed at the patient's particular emotional conflicts, will be followed by bursts of abnormal waves in the EEG. Both diffuse paroxysmal (spike-and-wave) and focal abnormalities can be so activated. Other epileptics and controls show only alpha blocking with or without muscle potential. Berlin and Yeager (16), and Higgins *et al.* (77), have reported similar observations.

The mechanisms of such responses are not known, but Darrow's hypotheses would appear to be pertinent.

Personality

Proposed relationships between brain-wave phenomena and normal personality traits or variables will be discussed here. EEG findings among patients with "functional" mental disorders are discussed in detail elsewhere (46).

In 1938 Gottlober (63) published data which appeared to indicate a positive relationship between extraversion and high alpha index (the percentage of time that a readable alpha rhythm is present in the EEG of a relaxed, awake subject). Henry and Knott (73) observed that Gottlober's sample was loaded with both extraverts and high alpha in-

dices. With additional data they were unable to find any relationship between alpha index and extraversion or introversion.

Saul *et al.* (162, 163) proposed several relationships between normal EEG patterns and certain personality characteristics: (*a*) Passive individuals tend to have regular, persistent alpha rhythms of high index, and "masculine competitive" individuals tend to have low voltage, weak alpha rhythms of low index. They defined passivity as connoting "dependence, submissiveness, the desire to receive from others, and readiness to retreat from dangers, effort, and responsibility. The meaning of the term can be defined further by contrast with the antithetical constellation of independence, drive, dominance, activity, and masculinity." (*b*) "(F)rustrated, demanding, impatient, aggressive, hostile women" tend to have "mixed" type EEG's (alpha index between 25 and 65 plus other waves both faster and slower than alpha) or "mixed fast" type EEG's (alpha rhythm over 10.5 cps plus waves faster than alpha).

The first proposition has been often cited in the literature (70, 117, 118, 142, 148, 159, 160, 161), and appears to have been accepted as fact. Sisson and Ellingson (171) reviewed the evidence upon which that proposition was based and found it unconvincing.

Rubin, Bowman, and Moses (142, 160, 161), subsequent to the original paper of Saul *et al.*, published EEG-personality studies on several groups of psychosomatic patients. These appeared to support a high-alpha-index-passivity relationship. These studies too were criticized by Sisson and Ellingson on methodological grounds and on the basis that the conclusions were based to some ex-

tent on circular reasoning.

There have also been some attempts to relate Rorschach scoring categories to the alpha index. Travis and Bennett (182) compared two groups of normals with alpha indices over and under 50. They found that the high-index group gave significantly more whole responses ($W\%$), but was significantly lower than the low-index group with respect to R, $Dd\text{-}plus\text{-}S\%$, sum C, and W, and took significantly more time.

Brudo and Darrow (22) found significant rank-order correlations between M and alpha index of .532 for 11 normal children, .636 for 10 behavior problem children with possible brain damage, and .619 for the two groups combined.

Sisson and Ellingson (171) compared two groups of 15 male neuropsychiatric patients with alpha indices over 90 and under 10, respectively, on 20 major Rorschach scoring categories. The groups did not differ significantly with respect to age or distribution of neuropsychiatric diagnoses. None of the differences between the two groups on Rorschach scores were significant (p's .30–.50). After discussing the limitations of using the Rorschach test in this manner to investigate relationships between personality and other variables, they concluded that "no study has been done conclusively showing a relationship between any feature of the normal adult EEG recorded under standard conditions and any personality trait or variable. . . . Since alpha and beta activity appear to be quite primitive functions of neural tissue, we find it difficult to believe that any of their measures will be found to correlate with any of the dimensions of so complex and phylogenetically recent an entity as the human personality."

Conclusion

It must be clear even from this review, limited to papers dealing with relationships between brain waves and psychological processes, that activity in the field of neurophysiology since World War II has been prodigious. Anatomists, physiologists, psychologists, neurologists, and psychiatrists have all contributed. Each new discovery seems to reveal the brain as an even more versatile organ than was previously appreciated. The major advances in the areas discussed have unquestionably stemmed from the delineation of anatomical connections and physiological functions of the reticular formation of the lower brain stem and the diffuse thalamic projection system. The research possibilities opened up by these discoveries have been far from exhausted.

It will have been recognized that while the body of confirmed and established facts relating EEG phenomena and psychological processes has been considerably increased since Lindsley's review of 1944, the body of unconfirmed data has likewise increased. Unconfirmed observations, especially impressionistic as contrasted with quantified ones, are more likely to be negated than confirmed. This situation is hardly unique, but caution cannot be overemphasized.

Theorizing too has proceeded apace. Not all of the theories offered can be correct. It is obvious, for example, that too many functions have been attributed to the alpha rhythm; it has perhaps received an undue amount of attention—a tendency which seems to be gradually diminishing. Some of the theories which have been outlined here are already obsolescent, but it can at least be said for most of them that they have inspired research.

Where the next few years will lead it is impossible confidently to predict. The mechanisms of the sleep-wakefulness cycle, of consciousness itself, are within our grasp. An approach is being made to the central nervous mechanisms of sensation, perception, and elementary motor activity. The so-called higher mental processes still appear to be beyond reach. Present neuropsychological techniques do not seem to be adequate to deal with them. Little practical assistance can be offered to the psychologist investigating the processes of learning or thinking or to the psychiatrist dealing with neuroses and psychoses, but in several laboratories efforts are even now being made to develop techniques to deal with these most complex of all problems.

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VICARIOUS TRIAL AND ERROR AND RELATED BEHAVIOR

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A number of experimenters have reported a more or less typical and frequent pattern of behavior which occurs at the point of choice in the discrimination box, maze, or during the choice process in visual discrimination studies employing jumping stands. This pattern has been variously described as "choice by negation or comparison," "looking to the right or left before choice," "swaying back and forth," "jumping toward and away from," "head movements," "partial elimination," etc. To this general pattern of behavior Muenzinger and Fletcher (**32**) have given the name "vicarious trial-and-error," abbreviated "VTE."

Although first used to label choice-point behavior of rats prior to spatial or nonspatial discriminative responses, the term VTE has subsequently been extended to vacillatory behavior in conditioning (**17**), reasoning (**27**), place vs. response learning, (**38**, **40**, **53**, **56**), delayed reaction (**24**), conflict (**2**), and even nonchoice (**43**) situations. Moreover, not only the choice-point or choosing behavior of rats but also of dogs (**17**), monkeys (**21**), children, (**2**, **15**, **16**), and adults (**51**) in various of these situations has been labeled "VTEing." Thus, VTE behavior now refers to the vacillatory behavior of various types of Ss at points of choice in a wide range of situations.

On the theroretical level, VTE has been interpreted as a behavioral definition of consciousness (**45**, **46**,

47), a catalytic process which aids learning (**48**, **49**, **51**, **52**), a form of symbolic exploration (**29**), an analogue or mechanism of reasoning (**26**, **30**), "overt thinking" (**9**), a behavioral index of conflict (**2**, **6**, **19**, **28**, **42**, **57**), or a preparatory response (**43**). Tolman (**48**, **49**, **51**), particularly, and Barker (**2**), Schlosberg and Solomon (**41**), Taylor and Reichlin (**43**), and Austin (**1**) have advanced relatively complex and systematic explanations of the VTE phenomenon.

On the empirical level, with the exception of the brief treatments of VTE by Muenzinger (**30**), Tolman (**48**, **52**), Dennis and Russell (**11**), Munn (**35**), Brogden (**5**), and Woodworth and Schlosberg (**62**), there has been no adequate systematic and critical analysis of experimental procedures and findings. Nor has the considerable work in this area resulted in an exegesis and comparison of the several theories of VTE behavior. Theoretical interpretations of VTE behavior, however, will be treated in a later paper. The purpose of the present paper is to summarize and critically evaluate the empirical material relating to VTE. The presentation and analysis will be divided into the following general topics: *criteria for VTE, antecedents and response correlates of VTE, and VTE and learning efficiency.*

CRITERIA FOR VTE

Descriptive Behavioral Manifestations of VTE

Muenzinger and Fletcher proposed

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the term "vicarious trial-and-error" as a label for the behavior "of white rats at the point of choice in a very difficult discrimination," where "the rats would stop and move their heads from alley to alley as if to gain a successive impression of the two stimuli to be discriminated" (**32**, p. 89). Later Muenzinger described this response alternation at the choice point as follows:

The most common way is for the rat to stop at a midpoint between the alleys and turn his head first toward one and then toward the other alley. But he may also approach the entrance to the alley and orient his whole body toward it and then turn and approach the other alley in a similar way. If electric shock is used as punishment he may stretch his body over the electric grid without touching it. Sometimes the movements may be even more extensive. After facing an alley he may turn back to the starting point and then run to the other grid (**30**, p. 77).

A number of investigators subsequently have employed Muenzinger's term, VTE, in referring to or describing selected aspects of choice-point behavior of the rat. For example, Tolman accepted the term VTE to designate the " 'lookings or runnings back and forth' which often appear at the choice point, and which all rat-runners have noted, but few have paid further attention to" (**48**, p. 27). It is interesting to note that some recent investigators (**6**, **28**), although concerned with similar patterns of choice-point behavior, have not used Muenzinger's descriptive term. On the other hand, Taylor and Reichlin (**43**) have attempted to show that behavior similar to that which Muenzinger and Tolman have designated as VTE occurs prior to jumps over a 6- to 8-inch gap in a nonchoice situation.

Up to the present time, no single or standard set of criteria (definitions) of VTE behavior has been established and used consistently.

This deserves special emphasis because a number of investigators using different defining and/or scoring criteria have made cross references apparently without recognizing that the comparability of these criteria has not been established. Furthermore, it has often been difficult to ascertain from reading a particular article what defining and/or scoring criteria were employed by the investigator.

Scoring VTE Behavior

VTEing or VTE-like behavior has been quantified or scored by criteria which will be designated here as VTE *units* or VTE *trials*. The VTE *unit* is defined as behavior which involves (*a*) looking at or facing one side or card and then turning toward and looking at (or facing) the other side or card before making a choice, or (*b*) looking at or facing one side or card then turning toward and looking at the other side or card and then returning to the first card before making a choice. The former is designated as the AB *unit*, the latter as the ABA *unit*.

The VTE *trial* is defined as any trial during which one or more VTE units were recorded; thus, the VTE trial may contain one or more AB or ABA units. As an additional semantic clarification, in this paper the general or unqualified terms VTE, VTE behavior or VTEing will be used occasionally as equivalent to nonquantitative descriptive phrases such as "rapid vibrations of the head," and "swaying back and forth," or as general categorical terms for either or both VTE trials or units.

It is difficult, if not impossible, to estimate the distortions or discrepancies in the interpretation of experimental data and in the comparisons

of different studies which may have been occasioned by the use of different criteria for scoring VTE. An examination by the present writers of data reported by Klüver (21) for weight, visual, and auditory discriminations of monkeys illustrates the difficulty more specifically. Although this investigator used VTE trials as the measure, in some cases his presentation permitted rescoring the data in terms of AB units. It was found that, during the acquisition of discriminative responses to weight stimuli, the AB unit and VTE trial appeared to be highly related since of all of the VTE trials reported approximately 90 per cent contained but one AB unit. Although it was difficult to ascertain this relationship for the visual and auditory discriminations, the impression was gained that a much smaller number of the VTE trials for these problems were scored on the basis of only one AB unit. This suggests that the size of the correlation between these different VTE measures may be in part a function of the particular experimental problem.

A re-examination in this connection of some VTE data obtained by Wischner (59, 60, 61) disclosed that for no-shock, shock-right, and shock-wrong groups in a black-white, non-correction discrimination situation, the percentages of VTE trials based on but one AB unit were respectively 83 per cent, 73 per cent, and 56 per cent. The rank-order coefficients of correlation between the number of VTE trials and the number of AB units were +.98 for no-shock, +.92 for shock-wrong, and +.86 for shock-right animals. Thus, while the particular experimental conditions apparently affected the percentages of trials which contained only one AB unit, fortunately, Wischner's data suggest little, if any, influence on the

correlation of VTE trial and AB unit measures.

The lack of a standard criterion of VTE, together with the relative lack of more precise information with regard to intercorrelations among the various measures, represent obvious limitations in any attempt to evaluate both empirical findings and any interpretations of these findings. These limitations must be kept in mind in subsequent discussions of reported relationships and interpretations.

As a first approach to a more precise quantification and understanding of VTE behavior, the present writers suggest that the AB unit, that is, the smallest unit, be adopted as the basis for scoring, since it would yield all data concerning the phenomena from which one could construct other VTE measures up to and including the VTE trial.[2]

[2] There are other considerations in scoring VTE behavior. One is the general problem of observing VTE's, for example, the occurrence of an AB unit. Thus, on some trials Wischner (60, 61) was unable to count and record each AB unit and so merely noted "much shifting." Therefore, in the above analysis the number of AB units for those trials was arbitrarily scored as two units. This probably represents an underestimate.

Such conditions as design of the entrance compartment, ease of delimitation of the choice point, spatial or cue characteristics of the stimuli to be discriminated, and extensiveness of the motor behavior involved in making the choice may all influence the ease of identifying VTE behavior. How to treat behaviors other than orienting toward or facing the stimuli which intervene among AB units within a trial represents a further consideration. To illustrate, during a single trial where A and B designate facing toward the positive and negative stimuli respectively a record may read, "ABA, return to entrance compartment, A, move to center, A, etc." While these behaviors may ultimately prove to be similar or equivalent to what Taylor and Reichlin (43) have termed preparatory responses, at present it would seem desirable to score these additional behaviors separately.

In a situation in which VTE behavior in

ANTECEDENTS AND RESPONSE CORRELATES OF VTE

Tolman is one of the few investigators who has systematically isolated and manipulated the variables which influence VTE. For the most part other relationships have emerged as incidental findings in experiments primarily concerned with other problems. Within various contexts and depending on the purpose of the writer the data from essentially the same studies have been subjected to three kinds of analysis. First, Tolman has been concerned with the nature of the relationships between VTE and errors in a variety of discrimination and maze learning situations. A second aspect has been the analysis of the relative frequency of VTE as a function of special experimental conditions without regard to the VTE-error relationship. Finally, relationships between VTE and other behavior measures have received attention.

The Relationship Between VTE and Errors in Learning

Tolman's extensive treatment of VTE-error relationships is apparently related to his postulation that VTEing leads to greater learning efficiency (45, 46, 47, 48, 49, 51, 52). While these relationships have been analyzed for both discrimination and maze learning, support for Tolman's hypothesis originated in and subsequently was drawn from discrimination learning data. In the following discussion the findings for these two learning situations will be treated separately.

children represented partial movements of a lever before a definite choice was made, Barker (2) weighted such movements for degree of displacement. It may be fruitful to develop techniques of scoring all types of VTE behavior in terms not only of frequency but also of amplitude.

Discrimination Learning

Yerkes, who described the VTEing (although, of course, he did not use this label) of dancing mice at the choice point in his discrimination set-up, commented that "could we but discover what the psychical states and the physiological conditions of the animal were during this period of choosing, comparative psychology and physiology would advance by a bound" (63, pp. 130-131). There is some suggestion in Yerkes' writing that, with easily discriminable stimuli, more frequent VTEing as exemplified by *choice by comparison* appeared first and, while remaining at a high level in some animals, in other animals decreased to choice by negation (one AB unit).

In other early investigations Hoge and Stocking (18) and later Lashley (23) observed that the frequent head movements of the early stages of discrimination learning in the Yerkes box gradually decreased in number as the discrimination was acquired. Conversely, Gellerman's (15) qualitative protocol for discrimination learning in young children indicated that head movements had appeared as correct responses increased in frequency. Pennington (36) reported that "choosing behavior" appeared after 400 trials as rats began to localize sounds and was most apparent during the last 100 trials of learning. He also indicated that Thuma (44) had noticed similar behavior, but the relationship of this behavior to errors was not explicitly specified. Drew's observation that "during these early trials 'vicarious trial and error' . . . appeared much more frequently with this group (shock at door of right choice) than with the others" (12, pp. 263-264) does not provide sufficient information to specify the course of VTEing. Similar

interpretive difficulties characterize Girden's report "that the elimination of paw flexion to LS proceeded rapidly with its [head and visual orientations to the left and right side of the apparatus] appearance" (**17**, p. 683).

Relationships between comparison behavior and error elimination have been reported for monkeys in Klüver's (**21**) string-pulling situation. Comparison behavior consisted of motor comparisons (successive pullings of strings) and visual comparisons (eye movements from one box to the other). In general, Klüver's data for weight, auditory, and visual stimuli suggested the existence of an inverse relationship between errors and frequencies of comparison or VTE trials.

Referring to data gathered in collaboration with his students (**31, 32, 33, 34**), Muenzinger (**30**) concluded that the frequency of VTE trials was highest when errors were almost eliminated.

The most complete data concerning the relationship between VTE and errors have been marshalled by Tolman. The relationship which he has presented most frequently both for groups and individual rats is a plot of the course of VTE behavior and errors during the learning process. Thus Tolman (**48**) has cited curves for black-white and tone discriminations obtained by Gentry in Muenzinger's laboratory, which revealed that errors had decreased as VTE trials had increased. Also, he has reported that as errors decreased VTE trials increased in discrimination studies employing brain lesion and normal animals, jump and no jump conditions, and near and far discriminanda (**48**).

Tolman (**49**) compared AB units, correct choices, and hesitation time for three experimental groups which

had been presented with white-black, white-medium gray, and white-light gray discriminations with white positive. While in the case of the easiest discrimination maximum frequency of VTE coincided with the first errorless day, it is interesting to note that the VTE curve for the white-medium gray group was highest on days 8 and 11 although the fewest errors did not occur until the eighteenth day. Both error and VTE curves for the white-light gray group were relatively flat.

When black-white discriminanda were separated by spatial angles of 30°, 80°, and 130°, Tolman (**50**) also reported an inverse relationship between AB units and errors during acquisition. In a study by Tolman and Minium (**54**) VTE frequency in AB units was highest on the seventh day although black-white discrimination learning was not completed until the twenty-second day. When animals were then required to learn black-light gray and black-dark gray discriminations, the highest frequencies of VTE units did not occur simultaneously with diminished error frequencies. Tolman and Ritchie's (**55**) observation of an inverse relationship between VTE and error curves was supplemented by another measure, namely the correlation coefficient, which they reported to be −.65 as between totals of errors and AB units for individual animals over all trials.

Analysis of VTE data compiled by Wischner (**59, 60, 61**) in a noncorrection brightness discrimination indicated that no-shock, shock-wrong, and shock-right conditions had differential effects on both overall frequencies of VTEing and relationships between VTE's and learning trials. The no-shock group VTEed on 12 per cent of the trials and had a mean of 0.15 AB units per trial. VTE's occurred on 22 per

cent of the trials with a mean of 0.45 AB units per trial for the shock-wrong animals. The values for the shock-right group were 36 per cent and 0.56 units per trial. A plot of frequencies of either VTE trials or units against tenths of trials for the no-shock group revealed low frequencies for the first half of the learning process and then a rapid and regular increase for the last half. For the shock-wrong condition, both measures increased rapidly to a maximum at the second or third tenth, declined nearly as sharply and then leveled off for the last three tenths. While trials and units also increased rapidly to the third or fourth tenth for the shock-right condition, the curves remained at fairly high levels for the remaining trials.

The relationship between VTEing and errors for these data may be expressed as the correlation between VTE trials or VTE units per tenth of trials on the one hand and errors per tenth of trials on the other, that is, the relationship between corresponding points for Vincent error curves and Vincent VTE curves (in tenths of trials). The following rank-difference correlation coefficients were obtained for each group for the two VTE measures, trials and units.

	VTE Trials	VTE Units
No-shock	−.61	−.84
Shock-right	+.34	+.26
Shock-wrong	−.16	+.56

These data suggest a positive relationship between errors and both VTE measures for the shock-right group. The data for the shock-wrong group are somewhat ambiguous suggesting only a slightly negative relationship for VTE trials and a relatively high positive relationship between errors and VTE units. Only

for the no-shock group do the data clearly indicate the inverse relationship between VTE's and errors which has been stressed in Tolman's writings. It would appear from these data that the nature of the VTE-error relationship in discrimination situations is dependent upon experimental conditions such as administration of electric shock.

Rank-order coefficients of correlation between various measures of VTE behavior (trials, total AB units, and AB units per trial) and learning measures (errors and trials) for individual animals of each group were also computed for both the total learning period and for the first 100 trials. Of the 27 coefficients obtained none of the six negative values (range of −.01 to −.33) and only two of the 21 positive values (range of +.07 to +.71) were significant at the .05 level. These data do not agree with the coefficient of −.65 between errors and VTE's for individual animals reported by Tolman and Ritchie (55).

For correction animals learning a horizontal-vertical bar discrimination in a modified Grice apparatus, Lane (22) reported the appearance of "much VTE behavior from the fifth day of training on until just before the animal learned the discrimination" which "coincided with a relatively constant increase in the positive reaction tendency" (22, p. 48). The slower learning noncorrection animals exhibited less VTEing which, however, also appeared when correct responses began to depart from chance levels on about the twelfth day of training. Unfortunately, no day-by-day record of VTE's was presented thus precluding a more adequate assessment of the VTE-errors relationships for the two conditions.

The preceding data were for the

acquisition phase of discrimination learning. During overlearning trials the nature of the VTE-error relationship is apparently dependent on the difficulty of the discrimination. Thus it has been observed that for easy discriminations, following attainment of the criterion, VTE frequency (AB units) decreases (54). When more difficult visual or auditory discriminations are required VTE frquency tends to remain at a high level during overlearning trials (48, 54).

Maze Learning

Peterson observed that after a gradual decrease in the distance of entrance into the blind alley, and "just before entrance is eliminated completely, there frequently occurs a peculiar and very rapid vibration of the rat's head between the direction of the true path and that of the tempting blind alley" (37, p. 52). In a later quantitative study of VTE behavior in the maze, Dennis (10, 11) reported that more head movements occurred in the second half of acquisition trials. Similarly, Crannell (8) observed an increase in hesitation time and VTEing as rats approached criterion in multiple-path mazes. Both measures decreased following achievement of the criterion.

"Back and forth running between the starting point and points beyond the first screen" apparently decreased with successive trials for "always" and "sometimes ran" animals in one of Maier's (26) maze-like reasoning situations. Jackson's (20) 16-unit elevated maze with jumps at the choice point was designed to simulate the jumping requirement in the Lashley apparatus. During acquisition he obtained a positive correlation of .53 between VTE frequency in AB units and errors in individual rats, a much larger correlation of +.78 between VTE's and errors on maze units, and parallel curves for VTE, error, and time measures.

VTE and error relationships have also been obtained in place vs. response learning mazes. Thus in three studies by Tolman and his collaborators (38, 40, 56) it has been noted that for both place and response learners, as errors decreased VTEing also decreased. Similar direct or positive VTE-error relationships were observed for 12 and 46 hour hunger drive place learners and for response learners under the same deprivation schedules (53).

Evaluation

For discrimination learning Tolman has advanced the generalization of an inverse relationship between VTE and errors, that is, VTE frequency tends to increase to a maximum at about the point where errors are nearly eliminated (48, 49, 51, 52, 55). On the other hand, he concludes that in maze learning there is a positive relationship between VTE and errors (55).

An analysis of the studies summarized above, however, suggests that there are data which contradict or are not completely consistent with Tolman's generalizations. In this connection reinterpretation of the nature of the problem for animals in the place vs. response learning maze will be suggested later in this section.

With respect to discrimination learning, it will be recalled that Yerkes (63), Hoge and Stocking (18), and Lashley (23) apparently did not observe more frequent VTEing as errors were eliminated. Wischner's (61) findings for shock-wrong and shock-right conditions are also contradictory as were either the lack of correlation or the positive trends in correlations between VTE measures and rate of learning. Furthermore,

the data for Tolman's (49) white-medium gray group and the Tolman and Minium (54) findings are not entirely consistent with Tolman's generalization.

In the maze, Maier's (26) and Jackson's (20) observations of a positive relationship between VTE and errors are not consistent with Peterson's (37), Dennis' (10, 11) and Crannell's (8) findings. These discrepancies may be due to the use of different types of mazes, different maze patterns, no jumps or jumps at the choice point, etc. Regardless of source, they raise doubts concerning the general applicability of the thesis of a parallel decrease of VTE's and errors in mazes.

Presumably because the positive VTE and error relationships for place and response learning problems were obtained in mazes, Tolman (53) has interpreted them as in agreement with his notion of different relationships in discrimination and maze set-ups. However, the use of mazes and the requirement of a place response notwithstanding, the conditions of the California studies (38, 40, 53, 56) of place learning appear to approximate nonspatial discrimination paradigms more closely than the spatial discriminations of most mazes. In the usual nonspatial situation the positions of the positive and negative stimuli relative to both room and animal are shifted and, regardless of location, the animal learns to approach one (positive) stimulus and to avoid or not respond to the other (negative) stimulus. In place learning the position of the animals relative to the positive and negative places (stimuli) is changed by moving the animal rather than the stimuli. As a consequence, on some trials the positive place or stimulus is on the left and on other trials on the right and the animal must learn to ap-

proach one place or cue and avoid the other. If place learning is a variant of discrimination learning, however, the above-noted observations of decrements in VTE units as errors decreased contradict the findings for more conventional nonspatial discrimination conditions.

Evidence in support of a nonspatial discrimination learning interpretation of place learning has been reported by Blodgett and McCutchan (3) who found that animals, trained to turn right and to approach a light disc simultaneously, would turn left following reversal of the disc cue. Ritchie, Aeschliman, and Pierce (39) also suggested that both turning responses, presumably based on kinesthetic cues, and place or approach-a-discriminable-positive stimulus responses were acquired under place learning conditions. Webb (58) hypothesized that, if place and response learning were conceived as discrimination behavior, the greater the difference in extramaze cues the stronger the approach response to the extramaze cue accompanying reward. In accordance with this interpretation, when discriminable extramaze cues were placed in opposition to turning responses, the greater the cue differences the higher the percentage of approaches to the positive stimulus.

VTE as a Function of Special Experimental Conditions

Similarity and other characteristics of stimulus conditions influence VTE-ing in various learning situations. Also relevant are motivation and conflict variables, position preferences, organic conditions, and cage-rearing.

Similarity and Other Characteristics of Stimulus Conditions

The influence of similarity of stimuli-to-be-discriminated on VTE

frequency has been investigated most extensively. Variations in VTE frequency as a function of spatial angle between discriminanda, stimulus modality, form of visual stimuli, distance of stimuli-to-be-discriminated, distance of the jump, length of delay of reaction or of choice, length of cul-de-sac, and maze rotation have also been noted.

Stimulus similarity. With respect to the role of stimulus similarity Yerkes observed that:

Whenever the conditions are difficult to discriminate choice by comparison occurs most frequently and persistently. If, however, the conditions happen to be absolutely indiscriminable . . . the period of hesitation rapidly increases during the first three or four series of tests, then the mouse seems to lessen its efforts to discriminate and more and more tends to rush into one of the boxes without hesitation or examination . . . (**63**, pp. 131–132).

The suggested relationship between similarity and VTE is, therefore, curvilinear, with relatively lower frequencies of VTE for both easy discriminations and "absolutely indiscriminable" conditions.

It will be recalled, however, that the "easy discrimination" black-white group of a previously cited study (**49**) learned the discrimination more rapidly and VTEed more than groups which had white-medium gray and white-light gray discriminanda. Accordingly, Tolman concluded that, during the acquisition of discriminations by animals, VTE frequency varied inversely with the similarity of visual (and by implication auditory, tactual, etc.) stimuli, that is, the less similar the stimuli-to-be-discriminated the more frequent the VTEing. However, these data do not support this conclusion unequivocally. Thus, because the experiment was terminated when the white-medium gray group was making only 75 per cent correct choices

and the white-light gray group was at chance levels, it is possible that these groups were not given enough trials to permit the occurrence of the frequent VTE's which, according to Tolman, just precede or accompany error elimination.

Tolman (**51**) has also referred to unpublished observations that in humans similarity has an opposite effect, that is, the greater the similarity of stimuli the more frequent the VTEing. This apparent species difference was attributed to the possibility that rats first had to "discover instructions" or learn what-to-do, whereas through instructions this prerequisite had already been met by humans. It would follow, therefore, that after rats had learned what-to-do the inverse relationship should change to a direct relationship with VTE increasing as similarity increases.

During test trials, administered after rats had learned a black-white discrimination, Schlosberg and Solomon (**41**) found a direct relationship between VTE (ABA units) and the degree of similarity of the stimuli-to-be-discriminated. Similarly, Brown (**6**) tested the strength of reactions, including the number of head movements (AB units), as a function of increasingly difficult discriminations after rats had learned to discriminate bright from dim. The group which had been shocked for errors during training and was one hour hungry during the test exhibited an increase and subsequent decrease in the number of head movements as positive and negative stimuli were made more similar. This trend was predicted, however, as the outcome of conflict between a weak approach response and a strong avoidance tendency. For the other groups increased difficulty of discrimination was accompanied by more frequent VTEing.

When retrained and then retested on the same discrimination, all groups exhibited increased head movements as the discrimination became more difficult. Because of different training procedures and because testing was done *after* the development of discriminations, these results are not directly comparable to the Tolman data noted above. Schlosberg and Solomon's and Brown's findings are consistent with Tolman and Minium's (54) observations for overlearning trials that, for difficult discriminations, VTE frequency did not decrease with overlearning but for easy discriminations there was a decline in VTE behavior.

Klüver (21) does not present sufficient data to evaluate the nature of the relationship between VTE behavior and the similarity of visual or auditory stimuli. For weights, however, he concluded that "in experiments requiring successive comparisons we made the general observation that a decrease in the stimulus difference does one of two things: it either abolishes the comparison behavior entirely or almost entirely, or it increases the number of comparisons" (21, p. 45).

Spatial angle between stimuli-to-be-discriminated. Tolman (50) found VTE frequency in units to be an inverse function of the 30°, 80°, and 130° angular separation of black-white discriminanda. The 30° group exhibited more VTE's and faster learning than either of the other groups, and longer choice time than the 80° group. More VTE's and fewer errors were recorded for the 80° group than for the 130° group, while the latter group required longer choice times than either the 30° or 80° groups. The longer choice times for the 130° group were explained as a tendency for those animals to remain "stuck" at a particular stimulus, and

to "forget" the alternative stimulus. More frequent and persistent VTE-ing after learning a black-white discrimination in a **T**-shaped than in a **Y**-shaped box has also been reported (30).

Stimulus modality. Klüver found that the relative frequency of visual comparisons was greater with visual than with auditory or weight stimuli. The Gentry data reported by Tolman (48) indicated that VTE frequency is higher for auditory than for visual discriminanda. Klüver's data for total frequencies of both visual and motor comparisons with auditory and visual stimuli appear to be consistent with this conclusion.

Type of visual form. Klüver (21), who has reported the only relevant data on the influence of types of visual form, found greater hesitation and more frequent comparisons with triangles and crosses than with squares, circles, hexagons, and irregular forms.

Distance of stimuli-to-be-discriminated. Tolman (48) reported an experiment carried out in his laboratory in which conditions were so arranged that the black-white discriminanda were 23½ in. away from the jumping platform for one group and 12½ in. away for another group. It was observed that VTE frequency in trials was considerably higher for the latter group. However, the 23½ in. group reached a level of only 75 per cent correct responses at the termination of training. It is possible, therefore, that termination of training prevented the appearance of a higher frequency of VTE as the animals in this group approached criterion.

Distance to jump. Schlosberg and Solomon have advanced the hypothesis that "anything that increases the general tendency not-to-jump (as, e.g., increased distance) will increase latencies, increase VTE,

and decrease errors" (**41**, p. 38). Tolman (**48**) found that in a black-white discrimination a no-jump group made considerably fewer VTE trials than a group which was required to jump an $8\frac{1}{2}$ in. gap between the jumping and landing platforms. Similarly, animals which had a longer jump from the small platform in McCord's (**24**) delayed reaction set-up "looked at" more doors during the last 60 trials than a larger platform-shorter jump group. It may be noted that because of differences in platform size the stimuli for the smaller platform group were also farther away.

Length of delay of reaction or of choice. McCord (**24**) reported that the greater the length of delay in a delayed reaction situation the more doors faced or examined. "A tendency to pause occasionally and regard one door or another" under delay response conditions has also been noted by MacCorquodale (**25**). Muenzinger and Fletcher ascribed the increase in learning efficiency resulting from enforced delay at the choice point to "the longer and at times successive facings of the stimuli to be discriminated" (**33**, p. 389).

Length of cul-de-sac. Dennis (**10**) found that frequencies of head movements and of partial eliminations were directly related to length of cul-de-sac.

Rotation of the maze. Following completion of place and response learning trials (**53**) VTE frequency increased slightly as the result of a 90° rotation of a **T** maze.

Motivation and Conflict Conditions

When generalized to other situations Schlosberg and Solomon's (**41**) thesis that VTE increases as a function of conditions which strengthen tendencies not-to-jump suggests that **strength of punishment might be** related to VTE behavior. Punishment presumably influences fear or anxiety as well as the strengths of the anxiety-motivated avoidance responses of various conflict situations. Therefore, VTE frequency might also change with variations in drive strength and types of conflict.

Punishment (electric shock). Muenzinger (**30, 34**) reported more VTE trials for shock-right and shock-wrong groups than for a no-shock group. After attainment of criterion VTE frequency of the more frequently and continuously punished shock-right group remained at a high level in contrast to the decline in VTEing of the shock-wrong group. Wischner (**59, 60, 61**) found that a shock-right group exhibited more VTE trials and AB units than both no-shock and shock-wrong groups, although the latter VTEed more than the no-shock group.

Brown (**6**) demonstrated that shifts in conflict situations from approach-approach, to double approach-avoidance (approach-avoidance tendencies to both stimuli of the conflict situation), to avoidance-avoidance, which involved increasing strength of punishment, led to increased frequencies of head movements. More vacillations occurred in the more punishing avoidance-avoidance situations of the Barker (**2**), Klebanoff (**28**), and Hunt (**28**) experiments than in approach-approach conflicts.

Fairlie (**13**) classified choice-point behavior in a black-white discrimination box as no-pause, look 1 (paused but head turned in one direction only), look 2 (looked one way and went the other), and look 3 (looked back and forth three or more times). His shock-wrong group made considerably more no-pause and less look 1 responses than the shock-right group; frequencies of look 2 and look 3 responses for the two

groups appeared to be more nearly equal.

Muenzinger and Fletcher (32) found that a group with hunger and escape from shock as motives made more errors and exhibited less VTE than a second group motivated by escape from shock alone. No food was given for the correct responses of either group. Because the former group made more errors, it was punished more frequently and hence should have made more VTE's. However, the presence of the hunger drive, which would presumably decrease VTE's (see below) and at the same time prolong exploration for the nonexistent food, might have accounted for the failure to obtain a higher frequency of VTEing for the dual motive group.

It would seem that VTEing is more likely to occur as responses are more frequently accompanied by punishing consequences. High hunger drive and other factors, however, may sometimes obscure this relationship. Drew's (12) report that rats shocked at the door for the right choice VTE more frequently than no-shock, shock-at-wrong-door, or shock-at-food groups suggests that the locus of administration of shock might be one of these additional factors.

Strength of (appetitional, aversive) drive. Both Brown (6) and Tolman and Gleitman (53) have reported data which are in accordance with the derivation that "anything that increases the general level of forward-going tendencies (e.g., increased hunger) will decrease latencies, decrease VTE, and decrease the number of errors" (41, p. 38). In a series of postlearning tests the former investigator found that animals run under conditions of 48 hours hunger drive made fewer head movements than animals one hour hungry. The latter found a lower frequency of VTE and

errors for place and response learning animals run under 46-hours food deprivation than for place and response learning animals 12 hours hungry.

As noted above, Muenzinger and Fletcher (32) reported that a group motivated by hunger and escape from shock made fewer VTE's than an escape-from-shock group. The presence of the hunger drive, with no food for any response, may have had the dual effect of inhibiting the expected higher frequency of VTE, which more frequent punishment because of errors would presumably have occasioned, while at the same time serving to increase errors by prolonging exploratory behavior. As a consequence, this study does not appear to be a critical test of the generalization that VTE's and strength of the hunger drive are inversely related. Sears and Hovland (42), who varied the strength of motivation for conflicting avoidance responses, found low percentages of *double* reactions (responses to one signal and then to the other) in all three of their groups of human Ss. A high proportion of blocking responses due to equality of response strengths was thought to account for the low percentage of vacillation for two groups; the infrequent vacillations of the third group were attributed in part to a disparity in the strength of the competing reactions. Intergroup differences in *double* reactions were not significant.

Type of conflict. In a study by Barker (2), children indicated preference for one of two liquids by moving a lever in the direction of choice. These lever movements, appearing as lines on recording paper, were weighted for extent of displacement and summed to indicate amount of VTE behavior. It was observed that the more equal the

preference for any two liquids, the greater the amount of VTE behavior. Moreover, VTE scores tended to be highest when the choice was between pairs of undesirable liquids. The latter finding is consistent with Brown's (6) observation of more head movements by rats in an avoidance-avoidance conflict than in double approach-avoidance or approach-approach conflicts. The previously cited findings of Klebanoff and Hunt are also in accord with the Barker and Brown results. Miller's (28) report that Godbeer's children Ss made more eye movements in double-approach-avoidance than in approach-approach situations adds further support to the conclusion that VTE behavior decreases as the type of conflict shifts from avoidance-avoidance to approach-approach. Hovland and Sears' (19) observation, again for human Ss, that more *double* responses occurred in an approach-avoidance conflict than under approach-approach or avoidance-avoidance conditions represents a slight exception to this generalization.

Position Preferences

VTE frequency and position preferences during acquisition of discrimination responses. Klüver (21, pp. 79-80), observed that monkey BR exhibited no comparison behavior in a weight discrimination during the trials on which it was responding in terms of a right position preference. Inspection of curves presented by Tolman (49) indicates that in the early stages of discrimination learning, if position habits were operating, there was very little VTEing. As the animals began to respond with fewer than chance errors, VTE frequency increased. Analysis of some of Wischner's (60, 61) data for a no-shock group also sug-

gests that VTE's were less frequent during the position habit days early in learning than on later days when position responses were decreasing in strength. Lane's (22) data can be interpreted in similar fashion.

VTE frequency when negative stimuli are on the position habit side. Wischner (60) has reported that for a shock-wrong and no-shock group respectively, 74 per cent and 84 per cent of the VTE trials occurred when the animals, as they advanced from the entrance compartment, happened to approach first the negative stimulus. For a shock-right group, however, this value was 42 per cent. These figures are for the total learning period. Corresponding percentages for the first 100 trials for the first two groups were 72 per cent and 70 per cent. For the shock-right group this value was only 29 per cent. It has been suggested (59, 60) however, that for the latter group, the no-shock alley might be considered "positive," particularly in the early stages of learning. These data suggest that the greater percentage of VTE trials occurred when the animal chanced to approach first the alley in which it received shock, or in the case of the no-shock animals, the alley in which it was simply frustrated (no food reward). Further analysis of no-shock group data suggested that these animals tended to face the negative stimulus first and VTE when it was presented on the position habit side. Therefore under the specific condition of the presentation of the negative stimulus on the position habit side VTE trials appeared to be directly related to position preferences; this relationship was most apparent as completed position responses decreased in frequency. Since a similar relationship was not observed for shock-wrong and shock-right groups, a more complicated

but as yet unspecified interaction of relevant variables is indicated.

Organic Conditions and Cage-rearing

Tolman (48) has reported Friedman's findings that normal animals made fewer errors and more VTE trials in learning to turn left in a simple **T** than animals with a moderate amount of cortical lesion. In another California study (48), after rats were blinded upon completion of place or response learning under 12 or 46 hour hunger drives, frequencies of VTE units were higher.

Cage-rearing as one type of pre-experimental experience apparently led to greater hesitation, freezing, and VTEing at the choice-points of a 9-unit **T** maze (7).

Response Correlates

Length of entrance into the cul-de-sac or wrong alley. Muenzinger (30) reported that for no-shock and shock-wrong groups VTEing was seldom followed by full penetration of the incorrect alley. However, the rats of the shock-right group when making an incorrect response continued to proceed to the end of the wrong alley during the first 100 trials. Peterson (37) noted that the highest frequency of head movements occurred when complete entrances, half-entrances, and starts into cul-de-sacs were disappearing and/or had disappeared. Dennis (10) has reported similar results. Thus, in general, VTE frequency would appear to increase as length of entrance into the cul-de-sac or wrong alley decreases. With further learning, however, VTEing should decrease and disappear.

Correct responses. Referring to Dennis' earlier study, Dennis and Russell noted that "runs in which head movements occurred were cor-

rect about twice as often as the average run or the immediately preceding run" (11, p. 307). Fairlie (13) observed that look 2 and look 3 responses made by the shock-wrong group were apparently related to a greater frequency of correct responses than the no-pause and look 1 responses; this pattern did not hold for the shock-right group. In Wischner's (60, 61) study, the least efficient shock-right group showed the most VTE's for both total learning and for the first 100 trials. Moreover, this group made correct responses on only 41 per cent of the trials on which VTE's occurred as compared with correct responses on 85 per cent and 89 per cent of the trials on which VTEing was observed for the shock-wrong and no-shock groups, respectively. For the first 100 trials the percentages for the latter two groups were 84 per cent and 73 per cent with the shock-right group now responding correctly on only 28 per cent of trials on which VTE behavior occurred. Wischner suggests, however, that if the "correct" response for the shock-right group is considered a response of avoiding the shock alley, VTE would then be related to "correct" choices.

Hesitation (choice) time. As might be anticipated, a number of investigators found that hesitation or choice time and VTE frequency were positively correlated in discrimination, trial-and-error, and delayed response situations (49, 20, 24). These findings are supported by the results of a factor analytic study of rats' behavior reported by Geier, Tolman, and Levin (14). Further, Barker (2) and Godbeer (28) employing children as *S*s found a positive relationship between VTE's and the time necessary to resolve conflict situations. VTEing and hesitation time were also

directly related in Crannell's (8) path-elimination maze.

One exception to the generality of the preceding results, the fact that the 130° animals in Tolman's (50) study had long reaction times but made few VTE's, might be explained by his hypothesis that the animals of the 130° group tended to get "stuck" at one stimulus and to "forget" the other. Other explanations are no doubt possible. A second exception, the breakdown in the positive relationship between head movements and time to run to the food cup for the first test series for Brown's (6) Group A (shocked for errors during training; tested with one hour hunger drive) was consistent with his theoretical formulation. Thus, while the positive relationship between VTE's and hesitation time holds generally, specific experimental and/or training conditions may require qualification of this relationship.

Other response correlates. With the exception of Group A in the first test series, Brown (6) found that after learning was completed, head movements, time to run to the food cup (hesitation or choice time), strength of pull in attempting to get out of the field, and distance run backward into the alley from the starting point were positively intercorrelated.

Taylor and Reichlin (43) argue that VTE is not limited to choice-point behavior in a discrimination situation. Postulating that VTE movements are preparatory responses which have lower thresholds than full responses, they used some of Hull's postulates to deduce several theorems describing changes in and relationships between various characteristics of the velocities of full and preparatory responses in a situation not requiring choice between alternatives. Velocity of full responses was defined as the reciprocal of reaction latencies. Velocity of preparatory responses was equal to the number of preparatory movements before the final response plus one divided by the total time the animal faced the stimulus. In general, the observed relationships between the forms, maximum standard deviations, and coefficients of variability of the distributions of full and preparatory responses agreed with theoretical expectations.

VTE AND LEARNING EFFICIENCY

Muenzinger (30) postulated that VTE behavior functioned as primitive thinking or rudimentary trial and error to facilitate acquisition of correct choices. Tolman's similar hypothesis, "that VTE's always aid the learning which they accompany" (48, p. 32) was later restricted to discrimination learning (55).

One set of data relevant to this hypothesis, relationships between VTEing and errors in discrimination and maze situations, has already been evaluated. Because of the pertinence of other findings, however, particularly when viewed in the perspective of the importance of the VTE and learning efficiency hypothesis in various versions of Tolman's system, final examination was deferred for additional development in this section. Criteria for the hypothesis will be noted before consideration of its adequacy.

Criteria for the learning efficiency hypothesis. The relationship to which Tolman has referred most frequently as supporting the VTE and learning efficiency hypothesis involves an increase in VTE measures to the learning criterion accompanied by a decrease in errors (48, 49). This relationship has been supplemented by

observations, often in the same experiments, that the groups which VTEed more frequently learned more rapidly. A negative correlation (55) between totals of errors and AB units for individual animals has also been employed as a criterion. (Because such totals are dependent on number of trials to criterion, AB units per trial is probably a more appropriate VTE measure.) The high percentage of correct responses on trials on which VTE's occurred has been a fourth criterion.

Some of Muenzinger's (30) and Tolman's (49) data suggest fairly close relationships among these criteria. However, Wischner's (59, 60, 61) study, in which all four criteria were determined, does not support high intercriteria correlations.

Evaluation of the learning efficiency hypothesis. Inconsistencies in the empirical data and the possibility of different interpretations of supposedly corroborative observations raise doubts concerning the general applicability of the hypothesis that, even in discrimination situations, VTEing per se aids learning. Thus it will be recalled that Yerkes' (63), Hoge and Stocking's (18), and Lashley's (23) early observations, the VTE curves for Wischner's (61) shock-wrong and shock-right groups, some of Tolman's data (49, 54), and, when interpreted as discrimination learning, the VTE and error relationship of the place learning task were apparent exceptions. Furthermore, while higher over-all frequencies of VTEing have accompanied more rapid learning in some discrimination studies (48, 49) this was not the case in Wischner's investigation. Finally, Tolman's complementary notion of parallel VTE and error curves in the maze (55) does not always hold (8, 10, 37). Data relevant to the first two criteria, therefore, are inconsistent for both discrimination and maze contexts.

The predominantly positive correlations or lack of correlation between various VTE measures and errors for individual animals during the first 100 and for all trials for Wischner's three groups did not corroborate the r of $-.65$ reported by Tolman and Ritchie (55). Both in the maze (10) and in discrimination situations (30, 59, 60, 61) regardless of the point in the learning sequence, the occurrence of VTE behavior on a given trial tended to be accompanied by a correct choice.

The general pattern of inconsistencies within situations and among criteria requires either the further restriction of the learning efficiency hypothesis or the development of a more satisfactory explanatory scheme. Following the first course, that of further restriction of the hypothesis, it can be noted that the contradictory data for discrimination learning were obtained in apparatus other than the Lashley jumping-stand. Therefore, it could be postulated that data favorable to the hypothesis are more likely to be obtained in jumping-stands. In this case, however, it would be necessary to explain Muenzinger's corroborative data for other situations and, more importantly, the pertinent special features of the jumping-stand.

The mazes in which contradictory evidence was obtained differed from those in which parallel VTE and error curves were obtained. Here, too, the hypothesis might be reformulated by the introduction of an, as yet, unspecified situational parameter(s).

Data or inferences from data reported by Tolman (49), Lane (22) and, particularly, by Wischner (59,

60, 61) suggest an interpretation of corroborative evidence which does not require the hypothesis that VTE-ing has a catalytic function in discrimination tasks. These data concern the relationship of position preferences to VTEing, particularly, for no-shock animals. Specifically, it is suggested that strong initial position preferences prevent the occurrence of VTEing during the initial phase of discrimination training. Later, as tendencies to go to positive stimulus plus left or right side, and to go to negative stimulus plus left or right side are more nearly equal, initial position preferences may continue to determine the side first faced. However if the negative stimulus is on that side, turns toward the positive stimulus should occur with greater frequency. With each occurrence an AB unit would be scored accompanied by a correct response.

This explanation suggests that any conditions, such as the jumping-stand, hard discriminations, and possibly very large spatial angles between discriminanda, which are likely to occasion strong position preferences, will also be marked by initial periods of infrequent VTEing. Then, as animals begin to respond to the positive cue with greater than chance frequency, VTE's are likely when the negative stimulus is on the position preference side.

It will be recalled that for Wischner's data it was found that shock-wrong and, with shock-avoidance interpreted as the "correct" response, shock-right animals tended to VTE when the negative cue was faced first, but such facings were not related to position preferences. Thus, an explanation based on position preferences also requires additional principles. Eschewing detailed development here, it is suggested that

analyses of conflict (**2, 6, 28**), particularly of the approach-avoidance type, approximate the requisite principles.

To conclude, the learning efficiency hypothesis seems neither generally applicable nor systematically powerful. Instead, the particular forms of VTE and error relationships can probably be more profitably conceived as dependent on combinations of conditions including position preferences and conflict. Theoretical elaboration of these relationships, however, is a matter for future consideration.

Summary

Three aspects of empirical data on VTE behavior have been considered: (*a*) criteria for VTE's (*b*) antecedents to and response correlates of VTE's, and (*c*) VTE's and learning efficiency.

Two criteria for scoring VTE behavior have been employed, VTE *units* which involve counting the number of times *S*s have faced or looked at the sides or stimuli of choice situations and the VTE *trial* defined as any trial during which one or more VTE units were scored. After noting the lack of standard criteria for VTE and the relative lack of information concerning intercriteria correlations under various conditions, it was suggested that one form of the VTE unit, the AB *unit*, should be recorded in all studies.

Relationships between VTE and errors in discrimination and maze learning were summarized and evaluated before consideration of VTE behavior as a function of stimulus characteristics, motivation and conflict variables, position preferences, organic, and cage-rearing conditions. VTE behavior was then related to length of entrance into the cul-de-sac, correct responses, hesitation time, initial facing or turning responses,

position preferences, and other response measures.

The Muenzinger and Tolman hypothesis that VTEing aids learning at least in discrimination situations was examined. Four criteria for the hypothesis were specified and examined in connection with given studies. In terms of these criteria the lack of consistent findings with

respect to VTE and error relationships in either discrimination or maze situations raised doubts concerning the learning efficiency hypothesis. An alternative explanation based on an analysis of the role of position preferences supplemented by principles of conflict was then advanced.

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BACKWARD CONDITIONING

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Three views are at present current among CR students with respect to backward conditioning. (*a*) Backward conditioning is a genuine variety of conditioning but such conditioning is not readily obtainable nor very pronounced, and in the course of usual CR training tends first to diminish and then to disappear and to acquire inhibitory characteristics (Pavlov's revised view, **18**, p. 393; earlier view—backward conditioning is impossible, *ibid*. p. 27).[1] (*b*) Backward conditioning is a genuine phenomenon but its very special characteristics, particularly that of diminishing through training, hardly entitles it to be classed as true conditioning (Spooner and Kellogg, **36**). (*c*) Backward conditioning is an artifact, resulting not from the CS-US pairings but from the US-sensitization or pseudo-conditioning (Bernstein, **4**; Grether, **9**; Harris, **11**; Osgood, **17**; Woodworth and Schlosberg, **42**).

Unfortunately, however, each of of the three views is based only upon a limited segment of "its own" evidence. The third view is in particular out of line with *total* evidence. And all certainly are badly in need of reconsideration in the light of: (*a*) Russian experiments that appeared after Pavlov's two books were published (1927 and 1928); (*b*) Russian experiments that were not considered by Pavlov since they were not performed in his own laboratory but in the labora-

tories of Beritov, Bekhterev, and Ivanov-Smolensky; and (*c*) the fact that by far most of the American experiments on backward conditioning used human *S*s and readily reportable and centrally controllable *R*s which could hardly make their data *alone* definitive with respect to conditioning in general. Moreover, it should be pointed out here that almost all discussions of backward conditioning seem to have overlooked the very essence of the problem; namely, the consideration that even unstable and temporary backward CR's are of great theoretical significance inasmuch as they obviously cast grave doubt upon any CR theory that makes "expectancy" the *sine qua non* to the formation of conditioning (to some extent they also reflect adversely upon "reinforcement" theories) and strongly advance "contiguity" and "contiguity plus" views.

The present review will thus attempt to offer both a critical evaluation and a theoretical integration of all the experimental evidence on backward conditioning. This experimental evidence consists, to date, of: (*a*) 13 experiments from Pavlov's laboratories (all with dogs);[2] (*b*) 4 experiments from other Russian laboratories (two animal and two human); and (*c*) 13 American experi-

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[1] The often-quoted statement of Pavlov that backward conditioning "is insignificant and evanescent" (**19**, p. 381; Russian: "neznachitel'nyy i skoroperekhodyashchiy," (**21**, Vol. 3, p. 92) must be translated as meaning "of small magnitude and short-lived."

[2] Only one (**13**) of the thirteen experiments from Pavlov's laboratories is discussed in some detail in Pavlov's English texts (**18, 19**). Two other experiments are mentioned only by name without any detail (they apparently had not been completed at the time of the writing of the texts), while the remaining ten are not mentioned at all (nine of the ten were performed after the texts were published).

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ments (nine human and four animal).[3] All the experiments from Pavlov's laboratories (some performed after his death) used dogs as Ss and, with the exception of one study in which an electric shock was the US, all were of classical design: that is, feeding as the US, salivation as the quantitative CR, and records of the animal's gross behavior as a general index of change. The "other Russian experiments" are: (a) two by Beritov, one on dogs and one on decorticated pigeons; (b) one by Shnirman from Bekhterev's laboratory with finger-withdrawal from electric shock in adult human Ss; and (c) one by Pressman from Ivanov-Smolensky's laboratory with "food-obtaining," "view-obtaining," and "verbal" techniques in school children. Finally, the American experiments comprise: (a) two early studies with white rats, (b) five experiments with the eyelid reaction in college students; (c) four experiments with finger-withdrawal from shock in college students; and (d) miscellaneous studies: one with four goldfish, one with two monkeys, and a preliminary attempt with the GSR and patellar reflex in college students.

[3] The two experiments by Nagaty (*J. exp. Psychol.*, 1951, **42**, 239–246; 333–340) are not included here. The responses used by Nagaty were not of a simple classical variety but of a complex instrumental nature: previously learned wheel rotating and bar pressing. Moreover, Nagaty did not study the *formation* of backward CR's but only the effects of backward CR trials upon previously formed forward CR's; more exactly, the effects of backward classical CR trials upon previously formed instrumental CR's (instrumental CR's can obviously be formed only in a forward manner). Hence, his experiments involve, in the writer's opinion, variables that are more complex than those under consideration in backward conditioning as such, and his negative results, while interesting in themselves, have little direct bearing upon the main thesis of the present review.

A definition of backward conditioning would also seem to be in order. And to a large extent the definition is rather simple. Backward conditioning is conditioning in which the conditional stimulus is activated a short time after the unconditional stimulus. The activation of the conditioned stimulus may furthermore occur either (a) a short time after the cessation of the action of the unconditioned stimulus or (b) a short time after the beginning of such action. In the latter case, however, one must be warned against confusing backward conditioning with "cessation conditioning" in which the conditional stimulus is activated a short time before the *end* of the action of the unconditional stimulus[4] (44) and is intended to produce not a conditional evocation but a conditional cessation of a reaction. Again, it should be noted that backward conditioning cannot refer to operant conditioning since in operant conditioning the conditional reaction, by definition, produces the unconditional one and obviously precedes it. Finally, it may need to be mentioned that backward conditioning should not be a priori bracketed with "backward association" in verbal learning, to which it may or not be related.

EXPERIMENTS FROM PAVLOV'S LABORATORY

The Krestovnikov experiment. This is the classical experiment, performed by Krestovnikov in 1913[5] and

[4] Psychologists who think of conditioning as effected between reactions rather than between stimuli will probably want to substitute "reaction" for "stimulus" in this and in the preceding two sentences. Such psychologists may regard some experiments which are included here as in reality cases of forward conditioning.

[5] Successful backward conditioning was reported in Pavlov's laboratory earlier by

published in detail in 1921 (13). The experiment was carried out in Pavlov's new laboratory—*E* in separate room, sound-proof animal room, automatic administration of unconditional and conditional stimuli, and capillary recording of salivation—and it was rather extensive, involving four to six months of experimentation with each one of five dogs. Moreover, all the five dogs had previous well-developed forward CR's—two dogs to the sight of a whirligig, one to a tactile stimulus, one to the odor of camphor, and one to the odor of amyl acetate. The backward CS's were as follows: for the two dogs with the whirligig CR's—a metronome of 68 beats per minute and a "loud" bell in one case, and the odor of camphor and a "mild" faradic shock in the other; for the dog with the tactile CR—a metronome of 100 beats per minute and a "loud" bell; for the dog with the camphor CR—a tuning fork of 3360∼ and a "loud" bell; and for the dog with the amyl acetate CR—the odor of vanillin. The backward delay—i.e., the time interval between the administration of the food (or 0.1% of HCl) and the application of the CS's—was 5 to 10 sec. in four dogs, and 5-10 and 2-3 sec. in the fifth animal.

Both Pavlov (18, p. 27) and Krestovnikov definitely state that the experiment showed absolutely no evidence of the formation of a backward CR. However, a detailed perusal of the results leads to a less categorical conclusion. True, two of

the five animals failed to reveal any signs of a backward CR even after several hundred backward reinforcements, and one of the two had his backward CS in the same modality as his forward CS—that is, his forward CR was to the odor of amyl acetate and he could not develop a backward CR to the odor of vanillin after 427 backward reinforcements in three and a half months of experimentation (Pavlov's laboratory seldom uses more than half a dozen reinforcements per day). Moreover, in these two animals, the hundreds of backward reinforcements did not seem to have any facilitatory or inhibitory effects upon subsequent formations of forward CR's, nor any facilitatory or inhibitory effects when the backward CS's were applied simultaneously or in close succession of CS's of the previously developed foward CR's. Yet, in the three other dogs, the protocols of the experiment do show backward conditioning, even though the conditioning might be regarded as unstable, sporadic, and of small magnitude. In two of the three dogs, the course of the development of the backward conditioning, interestingly, was quite similar to the one reported by Spooner and Kellogg (36) 34 years later: evident in the early stages of the training but diminishing and disappearing in later stages. But in the third of the three dogs, the developmental course was **V**-shaped: small in magnitude in early stages, absent in the middle stages, and reappearing in somewhat larger magnitudes in the last stage of training.

Nonetheless, Pavlov himself was apparently not fully convinced of the finality of Krestovnikov's negative results, as we note that in the early twenties he set a number of his students to reinvestigate the entire

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Pimenev (dissertation, 1907). However, Pimenev's experiment was performed in Pavlov's old laboratory and his data are incomplete so that not much significance should be attached to his findings. Pavlov himself mentioned Pimenev's results only in passing in one of his *Wednesday* seminars (20, Vol. 1, p. 156).

problem. What disturbed Pavlov was evidently not so much the impossibility of forming backward CR's as the reported fact that the backward CS's remained indifferent stimuli despite several hundred administrations while, according to Pavlovian "cortical dynamics," stimuli which do not become conditioned quickly acquire inhibitory tendencies. Hence, the task assigned to the students was to study "what happens in the cortex during backward conditioning" with an underlying objective of looking for the development of inhibition.

Experiments reported in 1927–1928. Anokhin (1) was the first to restudy backward conditioning but only in an exploratory manner. He used only 12 backward trials in one dog and was concerned not with the formation of backward CR's but with the aftereffects of backward combinations upon previously formed forward CR's to the same and to different stimuli. The forward CR's were to a light, a metronome, and a bell; and at first the light, the weakest CS, and then the bell, the strongest one, were applied once a day at a backward delay of 2 sec. instead of the usual 30-sec. forward delay. However, while the five backward light combinations reduced greatly the forward CR to the light and reduced considerably the CR's to the two other CS's, the seven backward bell combinations had no such effects. Anokhin explains his results by stating that the light being a weaker CS naturally induced a greater amount of inhibition, an explanation that obviously does not go too far.

Podkopayev (24) performed a much more extensive experiment lasting several months. He used two dogs, and studied primarily the for-

mation of backward CR's and only secondarily the aftereffects of such formations. His backward delay was 2 sec. and his backward CS's were a light in one animal and a thermal stimulus of 1°C. in the other. But here, too, the results were not uniform. No backward CR was formed to the light after a large number of backward trials but a backward CR of small magnitude was formed to the thermal stimulus after only a few backward trials. Moreover, the backward light combinations are reported to have inhibited the subsequent formation of a forward CR to the light, while the backward thermal-stimulus combinations are said to have facilitated the subsequent formation of a forward CR to the same stimulus. Podkopayev's data on the facilitation and inhibition are not too convincing since no direct evidence is available on what the course of the forward conditioning would have been without the preceding backward trials. But his results on the formation of the backward CR's themselves seem to be quite clear-cut.

Rite (32) introduced the methodological modification of forming first a forward CR to some CS, then "reversing the order" and studying backward experimentation with the same CS, and then trying the forward order again. She worked with two dogs, using a backward delay of 2–3 sec. and CS's of a light and a bell in one dog and of a light and a dampened tuning fork in the other. The results are here somewhat more consistent. In no case did the backward combinations, ranging from 49 to 82, abolish completely the forward CR's to the same CS's. But the magnitudes of the forward CR's were reduced considerably—from one half to nine-tenths of initial values—in

three of the four cases, while in the fourth case—the dampened tuning fork—the CR remained practically unaffected. However, unlike those in the two previously cited experiments, the backward combinations did not affect subsequent formations of forward CR's to the same CS's.

Soloveichik (35) worked with two dogs using the same methodology as Rite, but backward delays of 5 sec., and longer periods of total experimentation. He found that 82 backward trials abolished completely a forward CR to a light and that 53 such trials reduced a CR to a weak noise from 50 to 3 units of conditioned saliva per half-minute. However, a forward CR to a loud noise in the same dog was unaffected by 135 backward reinforcements, while in another dog 291 backward trials (about three months of experimentation) were needed to reduce a forward CR to a bell from 26 to 4 units of conditioned saliva per half-minute.

In short, the experiments performed in the early twenties, unlike the Krestovnikov study, showed that backward CR's, even though small in magnitude and unstable, could be formed under certain circumstances. Moreover, these CR's were by all signs *genuine CR phenomena and not instances of pseudo-conditioning since they were in no case elicited by stimuli that had not been paired with the US's.* On the other hand, the Russian claim that backward CR's specifically assume in their formation inhibitory characteristics has not been borne out, in the reviewer's opinion, by the experiments so far cited. Such characteristics manifested themselves as a rule only after a large number of backward combinations and the fact is that even a large number of forward reinforcements tend to reduce CR's and produce so-called "extinc-

tion through reinforcements" (Russian: "*Ugasheniye s podkrepleniyem,*" 31).

Experiments reported in 1933. Kreps (12) worked with one dog using a backward delay of 2–3 sec. and backward CS's of a thermal stimulus of 45°C. and of the odor of camphor. At first, no CR was in evidence after 100 backward reinforcements in 40 consecutive experimental days. But when the experiment was interrupted for a week and a number of the dog's previous forward CR's were reactivated to raise the level of "cortical excitability," a backward CR to both CS's was formed in 5–6 backward trials. The CR's were, however, quite unstable, and Kreps seems to have been the first E to show clearly that the backward CR's in his dog retarded substantially subsequent formations of forward CR's to the same stimuli.

Pavlova (22) worked with three dogs studying the effects of backward conditioning upon both the acquisition and the extinction of forward CR's. She used backward delays of "several seconds" and three separate procedures with appropriate controls to study their effects. (a) Backward combinations were rotated with forward combinations in the process of conditioning the dogs to a light and a whistle while only forward combinations were used in conditioning them to a tactile stimulus and to the bubbling of water. (b) Reinforced backward trials were substituted for nonreinforced trials in a partial-reinforcement technique in which a light as a CS was reinforced every third trial in a forward direction. (c) The extinction of a CR to a bell, in which reinforced backward trials were interspersed, was compared with regular extinction of a CR to a hissing sound. (The CR strengths of the experi-

mental and the control CS's as otherwise determined in the laboratory are said to have been equal in (*a*) and in (*c*).) Pavlova's data leave no doubt that, while the backward CR trials were by no means as effective as forward ones in producing conditioning, they certainly should be classed as positive CR variables inasmuch as they all aided considerably both the development of forward conditioning and the resistance of the conditioning to extinction.

Vinogradov (**39**) worked with two dogs that had no previous forward CR's. He used a backward delay of 15 sec., a combined US-CS activation of 15 sec., and a light, a metronome, a noise, a bell, and a tuning fork as CS's. *Backward CR's were formed in all cases and they all were of considerable magnitude.* However, they did develop more slowly and tended to diminish upon repeated reinforcements more than forward CR's. Thus, a backward CR to a metronome of 100 beats per minute first appeared after 12 reinforcements at a magnitude of 5 units of conditioned saliva per 30 sec. and continued to increase in magnitude until after 67 reinforcements it equalled a maximum of 30 units per 30 sec. But then it began decreasing and finally disappeared completely after 135 reinforcements. (Forward CR's to metronomes usually appear after 4–8 reinforcements, reach magnitudes of 50–60 units, and are likely to diminish only after several hundred reinforcements.)

Petrova (**23**) performed an extensive experiment with three dogs which had a number of previously formed forward CR's. She used a backward delay of 5 sec., a US-CS combination of 15 sec., and backward CS's of a hissing sound and of a rising white figure on a black background. Back-

ward CR's of small magnitude were formed in all cases but, as in Vinogradov's study, they, too, invariably began to diminish in the course of training and finally disappeared. However, Petrova continued to administer a large number of backward reinforcements after their CR's disappeared and found that these reinforcements, as well as nonreinforced applications of backward CS's, came to affect negatively not only the magnitudes of the animals' forward CR's but also the animals' total behavior. "Neurotic" disturbances, "hypnotization," and "paradoxical states"—strong stimuli producing week effects and weak stimuli strong effects—were produced in two of the three experimental animals.

Thus, the experiments reported in 1933 have brought out that (*a*) backward CR's could certainly be formed more readily than it was thought earlier, and that (*b*) the magnitudes of backward CR's may be comparable to those of forward CR's, but that (*c*) the backward CR's tend to diminish and disappear in the course of training much more commonly than do forward CR's. Again, the 1933 experiments indicate that the alleged inhibitory properties of backward CS's appear only after the backward CS's cease evoking CR's, and disclose the interesting finding that backward CR's are more readily formed with delays of 12–15 sec. than with those of 5 sec. and more with the latter than with delays of 2–3 sec.

Experiments reported in 1940. Nezhdanova (**16**) experimented with weak US's, the administration of 0.1% of HCl which was later strengthened to 0.3%. The backward delay was 3–5 sec., the backward CS was the bubbling of water, and the US-CS combination lasted 10 sec. Five trials were made each

day: four reinforced training trials and one nonreinforced test trial, and the intertrial intervals were always 5 min. The backward CR appeared after the twelfth reinforcement and continued to increase in magnitude in regular fashion. *There was no decrease in the backward CR after 445 reinforcements, and in all respects it behaved like a regular well-established forward CR.*

Stroganov (37) studied the effects of transforming forward CR's into backward ones in two dogs. He used a backward delay of 2–3 sec., bubbling water as a backward CS, and found that the effects differed with each dog. In one animal, the backward combinations diminished the forward CR while in the other they increased it. Stroganov explains his results in terms of Pavlovian typology, an explanation that has but little meaning when only two animals were used in the experiment.

Narbutovich (15) studied backward conditioning in two dogs in which a mild "unavoidable" electric shock was the US and a bell was the CS. He used two separate experimental procedures. In one dog, the shock lasted for 2–3 sec. and the bell was sounded for 2 sec. immediately after the shock ceased; whereas in the other dog the shock lasted for 2 sec., then was joined for 2 sec. by the bell, and then given alone for another 2 sec. Five trials were made each day: four reinforced training trials and one nonreinforced test trial. With the first procedure, a backward CR was formed after 19 reinforcements but disappeared after 47 reinforcements; with the second procedure, no evidence of any CR was noted after 56 reinforcements.

The experiments reported in 1940 have thus added two significant facts to the knowledge of backward conditioning; (a) the fact that an undiminishing backward CR of high magnitude could be formed under certain circumstances; (b) the fact that with electric shock as the US, a backward CR may be formed when the CS is applied immediately after the cessation of the US but not when it is applied during the action of the US.

OTHER RUSSIAN EXPERIMENTS

Beritov's experiments. Beritov experimented with two dogs (2) and with a decorticated pigeon (3). He used "unavoidable" electric shock as the US, and CS's of the sound of a metronome and of an organ pipe, and the flashing of a light. He seems to have been most successful with the decorticated pigeon in which a stable withdrawal CR was established by flashing the light 2–3 sec. after the cessation of the application of the shock. But a backward CR was also formed in one dog by sounding a tone of 512 on the organ pipe immediately or 1–3 sec. after the cessation of an electric shock of 2–3 duration to the animal's right forepaw. This CR developed quickly after 5 reinforcements but did not attain stability even after 100 reinforcements and was extinguishable in 2–3 nonreinforced trials. However, both Beritov's dogs failed to show any evidence of backward conditioning when (a) the shock lasted 30 sec. and a metronome was sounded 3–10 sec. after the beginning of the shock; and (b) a light was flashed 10–20 sec. after the cessation of a strong shock. Beritov's results thus corroborate those of Narbutovich (*supra*) that with shock as the US, backward CR's may be formed when the CS is applied after the cessation of the US but not when it is applied during the action of the latter.

Shnirman's experiments. The origi-

nal report of Shnirman's experiment was, unfortunately, unavailable to the reviewer who thus could only re-state Russian summaries that "Shnir-man worked with a large number of adult human Ss and found backward conditioning possible when the sound of a bell was applied 2–3 sec. after an electric shock as the US" (**2, 15**).

Pressman's study. Pressman (**26**) attempted backward conditioning in 14 school children—7–12 years of age —with the use of four different tech-niques: (*a*) finger-withdrawal from faradic shock; (*b*) so-called "Ivanov-Smolensky food-obtaining technique" in which child presses a rubber bulb to obtain some food, usually candy; (*c*) so-called "Ivanov-Smolensky orientating or view-obtaining tech-nique" in which the child presses the bulb to view some scenery; and (*d*) a "verbal technique" in which the child presses the bulb in response to E's command. The CS's were in all cases auditory stimuli—a bell, a crackling noise, and tones C and H on a Horn-bostel variator—and were always ap-plied immediately after the cessa-tion of the US—pressing the bulb or electric shock. The finger-withdrawal experiment lasted only for two ses-sions in which only 40 reinforcements were made, and its negative results could not be too conclusive. How-ever, the three other experiments con-tinued for hundreds of trials, and yet in no case was a stable backward CR established, while in most cases it was not formed at all. The children's spontaneous verbal reports of "What is the bell for?" "Say please what is the sense of the bell?" etc. are also of interest.

American Experiments

Early experiments with white rats. The first American study of what would now be called backward con-ditioning was performed by Carr and Freeman (**5**) with nine white rats. The rats failed to learn, in 1,500 trials, to "turn around and retrace a path" at the sound of a buzzer when the buzzer had been sounded each time approximately 1 sec. after the door of the path leading to food was closed, while two other groups of rats learned the association when the buzzer was sounded simultaneously with the closing of the door or ap-proximately 1 sec. before the door was closed (the performance of the "simultaneous" group being inferior, though, to that of the "1 sec. before" group). Yarbrough (**43**) modified the Carr and Freeman study by combin-ing an electric shock with the buzzer, which in a large way gave rise to quite a different learning task. His 38 rats first had to learn to turn around and retrace their pathway upon the ad-ministration of a shock, and then to do the same at the sound of a buzzer which preceded, followed, or was ap-plied simultaneously with the shock. Two "backward" conditions—buzzer following immediately the cessation of the shock and following it 1-sec. after—four "forward" conditions— buzzer 1 sec., 2 sec., 4 sec., and 6 sec. before the shock—and one "simul-taneous" condition were used with seven different subgroups of the 38 rats. And Yarbrough's results are quite different from those of Carr and Freeman. Both "backward" groups mastered the shock-buzzer associa-tion quite well. The "immediately after" group was almost as efficient as the "1 sec. before" group and the "1 sec. after" group was about 38 per cent as efficient as the "1 sec. before" group.

Eyelid reactions. In Cason's early experiment (**7**), one S was allowed to wink "naturally" for five hours during which "several thousand

winks must have been made," and these winks were connected with a telegraph sounder which clicked "when the natural wink was practically completed." Yet, 40 trials with the click alone did not produce any wink reaction. Cason duplicated his experiment 13 years later with 8 Ss who during two to four hours produced a total of 17,727 natural winks and were tested 304 times with the sound alone. But only 32 winks were elicited in the 304 tests and 21 of the 32 "occurred in the 2 Ss . . . who had the fastest natural winking rates." (8, p. 605).

Again, with "regular" CR procedures, Bernstein's (4) experiment shows only a small amount of backward conditioning, Switzer's (38) discloses a large amount of it, while Porter (25) maintains that he found no backward conditioning at all. Bernstein used a mild shock as the US, a click as the CS, and backward intervals of .5 and .9 sec. Switzer used a mechanical tap as the US, a buzzer as the CS, and backward intervals of .5–2.0 sec. And Porter used a puff of air as the US, a brief flash of light as the CS, and backward intervals of .47 and .98 sec. Bernstein suggested that some of Switzer's positive results might be due to "facilitation" (pseudo-conditioning) which may well be true. But it is very unlikely that all of Switzer's data could be accounted for in this manner. The fact is that the status of backward eyelid conditioning could hardly indeed be regarded as settled. No one of the three Es (leaving out the Cason study as a special case) continued his experiment for more than one session—maximum of 65 US-SC pairings—and all used adult human Ss and overlooked the consideration that backward conditioning may require special circum-

stances to be obtained and maintained and still be a genuine CR phenomenon. The entire problem is in need of reinvestigation on a larger scale and with a fresher outlook.

Finger-withdrawal from shock. Wolfle's two experiments (40, 41) with "avoidable" shock are well known. She used in her first experiment 10 Ss with a backward interval of .25 sec. and 10 with one of .5 sec., while the backward intervals in her second experiment were .2, .6, 1.0, and 2.0 sec. used with 24 Ss divided into groups of 5–7 Ss each. In the first experiment, the backward intervals of .25 and .5 sec. yielded respectively 10 and 13 per cent of conditioning as compared with 29 and 37 per cent obtained with forward intervals of the same duration. In the second experiment, the backward intervals of 1.0 sec. actually produced more conditioning than the forward intervals of 1.0 sec.—11 vs. 7 per cent—while backward intervals of .2 and .6 sec. produced only 7 and 10 per cent conditioning as compared with 52 and 37 per cent for forward intervals of the same duration. There is no doubt that Wolfle's experiments show backward conditioning, and the reviewer is by no means ready to attribute it all to pseudo-conditioning. However, larger-scale experimentation is needed here even more than in the case of the eyelid conditioning, since the conditioning of finger-withdrawal from shock is, as it known, notoriously variable (28).[6]

[6] The number of trials needed to form a shock-withdrawal CR in 21 human Ss in three Russian experiments was noted by the reviewer to range from 7 to 1,340. The mean was 213 and the SD was 255.7. American Es commonly report shock-conditionings in human Ss in terms of percentages of Ss conditioned in one session, and as a rule do not pursue the study of the CR processes in the

Harris (11) used 13 *S*s who "were acquainted with the essential facts of conditioning," a "loud tone" of 4.75 sec. duration which followed immediately a shock of .25 duration as the CS, and one experimental session of 80 US-CS pairings. A certain amount of backward conditioning was obtained which the reviewer, unlike Harris, believes could not justifiably be attributed "almost completely" to nonassociative factors or pseudo-conditioning by Harris' own data.

Spooner and Kellogg (36) no doubt performed the best controlled and the most extensive backward CR experiment in this country. They used *S*s who were "naive with respect to a knowledge of the psychology of learning and of the nature of conditioning," an "unavoidable" but adjusted shock intensity which always produced a 6-in. withdrawal movement, complete polygraph recordings of not only stimuli and responses but also of latencies, and an experimental session which lasted for two hours and was divided into five "experimentation blocks" separated by 5-min. rest-periods. The backward intervals were .25 sec. with one group of 10 *S*s and .5 sec. with another 10 *S*s, and their results certainly warrant their conclusion that "backward conditioning exists and that it must be accepted as an established fact" (*ibid.*, p. 328). On the other hand, the reviewer does not concur with the Spooner and Kellogg statement that *"backward conditioning is apparently an entirely different phenomenon from forward conditioning"* (*ibid.*; ital. in text). The statement seems to be based primarily upon findings that

the backward CR's had shorter latencies than the forward CR's—mean of .280 sec. for backward intervals of .5 sec. vs. mean of .491 for forward intervals of the same length —and that the backward CR diminished in frequency in the course of usual CR training. However, neither of these would seem to justify divesting backward conditioning of its rightful name. Latencies of CR are functions of CS-US asynchronism and there is no reason why radically different asynchronisms should not produce radically different latencies. The fact is that the mean latency of the CR's with forward intervals of 1 sec. was .750 which means that it differed more from the mean latency of the forward CR with .5-sec. intervals than the latter differed from the mean latency of backward CR's with .5-sec. intervals. And with respect to CR diminution with training, it will be remembered that the backward CR's in the early Russian experiments also manifested such diminution but that later adjustments of lengths of backward delays and strengths of CS's and US's proved the diminution to be not an invariable characteristic of backward conditioning.[7]

Miscellaneous experiments. Grether (9) established a backward "emotional" CR in two monkeys by using an explosion of flash powder or a "snake blowout" as the US and a bell as the CS; but then found that his two control monkeys formed the same CR through pseudo-conditioning. Similarly, Harlow (10) showed pseudo-conditioning to be as effec-

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nonconditioned *S*s. Russian *E*s have been known to continue their shock-conditionings in human *S*s for several months.

[7] Through an oversight, the experiment by Fitzwater and Reisman missed being included in the present review. Fitzwater and Reisman used avoidable finger-withdrawal in ten college students, and found little evidence for backward conditioning.

tive as, if not more effective than, either backward or forward conditioning in four goldfish when a strong shock was used as the US and a mild shock as the CS. Both these experiments are of prime significance in focusing the need for "pseudo-conditioning controls" which Pavlov's laboratory recognized years ago. But they obviously shed no light on the problem of the specific existence and nature of backward conditioning. Their stimuli and subjects—very strong US's in one case and animals low in phyletic scale in the other— are indeed too special to warrant generalizing their data to other more usual CR situations. Finally, it might be mentioned that Switzer (38) reported in a preliminary study successful backward patellar conditioning in 2 of 5 Ss and failure of backward GSR conditioning in 3 other Ss, and that Cason's early pupillary conditioning (6) was really backward in essence, the bell following the beginning of the action of the light.

INTEGRATIVE DISCUSSION AND THEORY

The sum total of the reviewed experimental studies leaves no doubt that backward conditioning is a genuine CR-associative phenomenon that is obtainable and maintainable under special conditions. The positive evidences for it are not only more numerous but, as revealed in the experiments of Vinogradov and Nezhdanova, clear-cut, otherwise unaccountable, and a product of months-long observations on animal Ss. The negative evidence, on the other hand, as found in the experiments of Porter and of Bernstein comes from short one-session studies with adult human Ss that could hardly be regarded as either definitive or exemplary with

respect to conditioning in general. Hence, the hypothesis advanced by a number of American experimenters and writers in this area, that backward conditioning is an artifact of pseudo-conditioning, might as well be rejected outright. The hypothesis would probably not have been offered if the essence of the reviewed Russian findings were known in this country. Moreover, a tenable argument can be made for not accepting the hypothesis even on the basis of available American data. The best-controlled American experiment with human Ss, that of Spooner and Kellogg, and the only American animal experiment in which standard animals and standard CS's and US's were used, that of Yarbrough, do not support it.

Nonetheless, it is also obvious in the cited studies that backward conditioning presents difficulties and requires special conditions for formation and maintenance. As so far noted, the special conditions for formation appear in the main to be: (a) a US that is not too strong (Nezhdanova, 16) as well as a CS that is not too weak (Podkopayev, 24); (b) with food as the US, a US-CS delay of some length (5–15 sec.; Soloveichik, 35; Vinogradov, 39); and (c) with shock as the US, applying the CS after the cessation and not during the action of the US (Beritov, 3; Narbutovich, 15). More special conditions might of course be revealed in future experiments but the present ones certainly seem to be little in accord with either a CR theory of "drive reduction" or one of "contiguity" in which coincidence of UR and movement-produced stimuli is assumed, not to mention "cognitive and expectancy" CR theories which, as indicated earlier, are in general out of line with any backward condition-

ing. Indeed, about the only view which the special conditions of backward conditioning would fit is the one of "dominance-contiguity" which the reviewer suggested some years ago (**28, 29**) and which in essence is only a behavioral reconceptualization of the doctrines of Pavlov and of Ukhtomsky and in a number of respects not unrelated to Lashley's concept of "what is associated" (**14**) and to F. Sheffield's discussions of consummatory behavior and "drive induction." According to this view, (*a*) conditioning requires not only a minimum of US and CS strength but also an optimum strength-ratio between the two: the US must be considerably stronger than the CS to dominate it but not so much stronger as to completely "ground" it; and (*b*) in CR situations, the dominance of a stimulus is a function not only of its intensive but also of its temporal characteristics: precedence in time enhances dominance greatly, while duration gradually lowers it. Hence, (*a*) the difficulties encountered in backward conditioning may be conceived of as being due to a US that is overdominant through combining its own intensive dominance with the dominance of precedence in time; and (*b*) conditioning may be effected when the US is either comparatively weak to begin with or becomes weakened through duration (with food as the US) or cessation (with shock as the US). (In Pavlovian terms, it means that the US must be strong enough to produce irradiation of excitation but not so strong as to produce negative induction, while subjectively we may say that in backward conditioning *S* must not be so much preoccupied with the US that the CS is not attended to.)

On the other hand, the diminish-ability of backward conditioning in the course of usual CR training, which while not an invariable characteristic (**16**) is still probably more common with backward than with forward CR's, might be interpreted in two ways. First, it may be argued that the formation of backward CR's of a CS-US direction is often paralleled by a formation of "reverse" forward CR's of a US-CS type—that is, the US's become CS's and the CS's, US's—and that these "reverse" forward CR's come to interfere with the backward conditioning. Both Beritov (**3**) and Bernstein (**4**, pp. 192–194) clearly demonstrated that the shock stimuli (the US) in their experiments came to evoke sound (CS) reactions, and the many instances of reduced UR's in the backward conditionings of Pavlov's laboratories are no doubt further illustrations of this characteristic. Second, it may be contended that at least in human *S*s the perception of the stimulus relations in the CR situation arising in the course of CR training tends to reduce and nullify the formed backward CR's. Or, in other words, while the acquisition of backward conditioning is assumed to proceed along noncognitive S-R lines, its extinction may well be conceived of as involving cognitive-perceptual factors. To be sure, the extinction of backward CR's is more often than not gradual in development. But this by itself could not be held as an objection to the present assumption since in uninstructed *S*s (*S*s whose attitudes to, and cognition of, the CR situation is not controlled by special instructions) the manifestation and effectiveness of cognition in conditioning is in itself often progressive in nature. Lastly, it should be noted that the mechanism that may underlie the two interpretations—(*a*) re-

verse conditioning interference and (b) cognitive factors—are not likely to be mutually exclusive, so that either or both may well be operative in any particular CR situation.

SUMMARY

1. Data of 13 experiments from Pavlov's laboratory (all with dogs), 4 experiments from other Russian laboratories (two animal and two human), and 13 American experiments (nine human and four animal) on backward conditioning were analyzed and evaluated.

2. On the whole, the analyzed evidence is unmistakable in demonstrating that backward conditioning is not a case of pseudo-conditioning but is a genuine CR-associative manifestation, and that stable backward CR's can be obtained and maintained under favorable experimental conditions.

3. Roughly, the conditions favorable for the formation of backward CR's are a US that is not too strong and a CS that is not too weak. With food as the US, backward conditioning is more readily obtained when the US-CS interval is 15 sec. than when it is 5 sec. and more readily with 5 sec. than with 2 sec. intervals. With shock as the US, backward conditioning seems to be possible only when the CS is applied after the shock has ceased and not when it is applied during the action of the shock.

4. In general, the main evidence on the formation of backward conditioning—both the favorable and the unfavorable factors—does not fit well the CR theories of either Guthrie or Hull or Tolman. It could, however, be accounted for by the writer's "dominance-contiguity" view which presupposes a favorable US-CS ratio of strength for effective conditioning—that is, the US must be dominant but not excessively dominant over the CS; or, in somewhat subjective terms, the organism must not be so much preoccupied with the US as to fail to attend in some degree also to the CS.

5. Backward conditioning is more subject to "extinction through reinforcement" than is forward conditioning, especially in the case of human Ss. A possible explanation for the phenomenon might be either that (a) Ss develop in the course of the backward conditioning a counter perception which centrally reduces the conditioning, or that (b) "reverse" conditioning is set up—CS's becoming US's and US's becoming CS's—as the temporal dominance of the CS's exceeds the intensive dominance of the US's. Experimental evidence for the operation of both mechanisms in conditioning has been long known.

6. The fact that (a) with food as the US backward conditioning is more effective with 15 sec. US-CS delays than with shorter intervals and that (b) with shock as the US it is effective only when the CS is applied after the shock has ceased, may well be taken as evidence against any assumption that backward CR's are merely forward CR's to traces or aftereffects of CS's.

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THE OPERANT CONDITIONING OF
HUMAN MOTOR BEHAVIOR[1]

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A very large body of experimental results have accumulated in the field of operant, or instrumental, conditioning of the rat, the pigeon, and of other experimental animals. The application to human behavior of the laws generated by such research is most often done by the use of theory. An alternative method is to demonstrate that the manipulation of classes of empirically defined variables that produce specific and highly characteristic changes in the behavior of small experimental animals in Skinner boxes produce similar changes in the behavior of college students.

This paper reports procedures for the direct application of the variables defining the paradigm for operant conditioning to human behavior, and shows that human beings act very much indeed like experimental animals when they are subjected to the same experimental treatments. It suggests that direct application of conditioning principles to some categories of human behavior may be justified. The procedures are simple, and they may be followed by anyone, with a minimum of equipment.

That it is possible to condition human motor behavior will surprise few who are concerned with behavior theory. Nevertheless, it has not always been clear what behaviors will

act as "responses," what events will prove to be "reinforcing stimuli," or exactly what procedures would most readily yield reproducible results. This paper describes methods that have been worked out for easy and rapid operant conditioning of motor behavior in humans, states characteristic findings, and reports sample results. Developed in a series of exploratory experiments in an elementary laboratory course in psychology, the methods may have a wider utility.

Development of the Method

In one year's class in the introductory laboratory, an attempt was made to reproduce the Greenspoon effect (1), in which the rate of saying plural nouns is brought under experimental control by the use, as a reinforcing stimulus, of a smile by the experimenter, or by his saying "Mmmm," or "Good." The results were indifferent: a few students had good success with some subjects; the majority failed with all their subjects. The successful students seemed, casually, to be the best-looking, most mature, most socially acceptable; they tended to have prestige. This suggested that the procedure was effective if S "cared" about E's behavior; that is, if he noticed, and responded in one way or another to what E said or did.

This observation is consistent with the Guthrian (but Skinner-box-derived) view that if one could isolate any single property shared by reinforcing stimuli (whether "primary" or "secondary"), it would prove to

be that all reinforcing stimuli produce a vigorous response of very short latency (2). Greenspoon's procedure was therefore modified to force S to respond to the stimuli that E wished to use as reinforcers. Thereafter, the incidence of failures to condition human Ss dropped considerably.

Using these methods, many kinds of stimuli have been found to be reinforcing in the hands of student experimenters, and a wide variety of responses have been conditioned. Data have been gathered on performance under regular reinforcement, and under such other schedules as variable and fixed interval, and variable and fixed ratio (3, 4), both in establishing rates of response and in yielding extinction curves of appropriate form after the termination of reinforcement. Experiments have been done on response differentiation, discrimination training and chaining. Indeed, there is reason to believe that the whole battery of operant phenomena can be reproduced in a short time. Incidental data have been obtained on "awareness," "insight," or what-have-you.

Here is a sample set of instructions to E for human conditioning. In presenting the method more fully, we shall amplify each section of these instructions in turn.

Procedure: Human Operant Motor Conditioning

1. Instruction to subject: "Your job is to work for points. You get a point every time I tap the table with my pencil. As soon as you get a point, record it *immediately*. You keep the record of your own points—try to get as many as possible." As necessary: "I'm sorry, I can't answer any questions. Work for points." DO NOT SAY ANYTHING ELSE TO S. Avoid smiling and nodding.

2. Reinforcing stimulus: pencil tap.

3. Response: tapping forefinger to chin. Be sure the tap on the chin is complete before reinforcing—that is, be sure that S has tapped his chin and withdrawn his finger. During regular reinforcen.ent, be sure S does not "jump the gun" and record a point before you give it to him. If S does this, withhold reinforcement and say: "You got no point that time. You get a point *only* when I tap the table. Be sure you get a point before recording."

4. Procedures: Observe S; determine operant level of chin-tapping before giving instructions.

a. Approximation conditioning of chin-tap (described later).

b. 100 regular reinforcements of chin-tap.

c. Shift to:

[⅓ of the subjects] 30-second fixed interval reinforcement.

[⅓ of the subjects] fixed ratio reinforcement at ratio given by S's rate per 30 seconds.

[When shifting from regular reinforcement to the schedule, make sure that S doesn't extinguish. If his rate has been high, you'll have to shift him, perhaps, to a 20:1 ratio—with such a change, S will probably extinguish. Prevent this by shifting him first to a 5:1 ratio (for 2 minutes), then to 10:1 (for 2 minutes), then to 20:1. Similarly, put S on 10-second F. I., then a 20-second F. I., and finally on a 30-second one.]

Continue for 500 responses.

d. Extinguish to a criterion of 12 successive 15-second intervals in which S gives not more than 2 responses in all.

5. Subject's "awareness":

[¼ of S's] Record any volunteered statement made by S.

[¼ of S's] At the end of the experiment, ask, "What do you think was going on during this experiment? How did it work?"

[¼ of S's] Add to instructions: "When you think you know why you are getting points, tell me. I won't tell you whether you're right or wrong, but tell me anyway." At about the middle of each procedure, ask, "What do you think we are doing now?"

[¼ of S's] At the beginning of each procedure, give S *full* instructions:

a. "You'll get a point every time you tap your chin, like this." (Demonstrate.)

b. "From now on, you'll get a point for every twentieth response," or " . . . for a response every 30 seconds." "From now on, you'll get no more points, but the experiment will continue."

6. Records:

a. Note responses reinforced during approximation; record time required, and number of reinforcements given.

b. Record number of responses by 15-second intervals. Accumulate.

c. Draw cumulative response curves.

d. Be sure your records and graphs clearly show all changes in procedure, and the points at which *S* makes statements about the procedure.

e. Compute mean rates of response for each part of the experiment.

f. Record all spontaneous comments of *S* that you can; note any and all aggressive behavior in extinction.

General Notes

Duration and situation. As short a time as 15 minutes, but, more typically, a period of 40 to 50 minutes can be allotted to condition an *S*, to collect data under regular and partial reinforcement schedules, to develop simple discriminations, and to trace through at least the earlier part of the extinction curve. The experiment should not be undertaken unless *S* has ample time available; otherwise *S*s tend to remember pressing engagements elsewhere when placed on a reinforcement schedule. We have not tried, as yet, to press many *S*s very much beyond an hour of experimentation.

The experiments can be done almost anywhere, in a laboratory room, in students' living quarters, or in offices. Background distractions, both visual and auditory, should be relatively constant. Spectators, whether they kibitz or not, disturb experimental results.

The *E* may sit opposite *S*, so that *S* can see him (this is necessary with some reinforcing stimuli), or *E* may sit slightly behind *S*. *S* should not be able to see *E*'s record of the data. In any case, *E* must be able to observe the behavior he is trying to condition.

Subject and experimenter. Any co-operative person can be used as a subject. It does not seem to matter whether *S* is sophisticated about the facts of conditioning; many *S*s successfully conditioned, who gave typical data, had themselves only just served as *E*s. However, an occasional slightly sophisticated *S* may try to figure out how he's "supposed to behave" and try to "give good data." He will then emit responses in such number and variety that it is difficult for *E* to differentiate out the response in which he is interested.

People who have had some experience with the operant conditioning of rats or pigeons seem to become effective experimenters, learning these techniques faster than others. The *E* must be skilled in delivering reinforcements at the proper time, and in spotting the responses he wants to condition. With his first and second human *S*, an *E* tends to be a little clumsy or slow in reinforcing, and his results are indifferent. About a third of our students are not successful with the first *S*. Practice is necessary.

Apparatus. The indispensable equipment is that used by *E* to record—a watch with a sweep second hand, and paper and pencil. Beyond these, the apparatus man can have a field day with lights, bells, screens, recorders, and so on. This is unnecessary.

Instructions

Conditioning may occur when *no* instructions whatever are given, but it is less predictable. The instructions presented here give consistent success.

Subjects may be told that they are participating in a "game," an "experiment," or in "the validation of a test of intelligence." All will work. Spectacular results may be achieved by describing the situation as a "test of intelligence," but this is not true for all *S*s.

In general, the simpler the instructions the better. No mention should be made that *S* is expected to *do* anything, or to *say* anything. Experience suggests that if more explicit

instruction is given, results are correspondingly poor. Elaborate instructions tangle S up in a lot of verbally initiated behavior that interferes with the conditioning process.

The instructions will be modified, of course, to fit the reinforcement. It seems to be important for S to have before him a record of the points he has earned. (This is not, of course, E's record of the data.) It seems to be better if he scores himself, whether by pressing a key that activates a counter, or by the method described here. Most Ss who do not have such a record either do not condition, or they quit working.

Reinforcing Stimuli

Any event of short duration whose incidence in time is under the control of E may be used as a reinforcing stimulus if S is instructed properly. The most convenient is the tap of a pencil or ruler on a table or chair arm, but E may say "point," "good," and so on. Lights, buzzers, counters, all work. One student found that getting up and walking around the room and then sitting down was a very effective reinforcer for his instructed S. ("Make me walk around the room.")[2]

The E may assign a "value" to the reinforcing stimulus in the instructions—e.g., for each 10 points S gets a cigarette, a nickel, or whatever. Members of a class may be told that if they earn enough points as Ss, they may omit writing a lab report.

Where no instructions are given, or where the instructions do not provide for an explicit response to a reinforcing stimulus (as in the Green-

spoon experiment—i.e., when E wishes to use a smile, or an "mm-mm," with the intention of showing "learning without awareness") many Ss will not become conditioned.

The most important features of the operation of reinforcement are (a) that the reinforcing stimulus have an abrupt onset, (b) that it be delivered as soon as possible after the response being conditioned has occurred, and (c) that it not be given unless the response has occurred. Delayed reinforcement slows up acquisition; it allows another response to occur before the reinforcement is given, and this response, rather than the chosen one, gets conditioned. The best interval at which to deliver a reinforcing stimulus seems to be the shortest one possible—the E's disjunctive reaction time.

When S has been conditioned, and is responding at a high rate, he may show "conditioned recording"—i.e., he will record the "point" before E has given it to him. The E must watch for this.

When S can observe E, it is entirely possible that S's behavior is being reinforced, not by the chosen reinforcing stimulus, but by others of E's activities, such as intention movements of tapping the table, nods of the head, and recording the response. The effect of such extraneous reinforcers can be easily observed during extinction, when the designated reinforcing stimulus is withdrawn. The precautions to be taken here will depend upon the purpose of the experiment. The E should thus remain as quiet and expressionless as possible.

The Response

The E has great latitude in his choice of behavior to be conditioned. It may be verbal or motor, it may be a response of measurable operant

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[2] Compare with the *Columbia Jester's* rat, who remarks to a colleague at the bar of a Skinner box, "Boy, have I got this guy conditioned: every time I press the bar, he gives me a pellet."

level before reinforcement, it may be a complex and infrequent response that *S* seldom, if ever, has performed. One qualification is that the response must be one that terminates relatively quickly, so that reinforcement can be given. (One *E* conditioned an *S* to bend his head to the left, reinforcing when the head was bent. The *S* held his head bent for longer and longer times, and so got fewer and fewer reinforcements as the procedure became effective. He became "bored" and stopped working.)

Motor behavior. The *E* may observe *S* for a few minutes before proposing to do an experiment on him, and choose to condition some motor behavior *S* occasionally shows, such as *turning his head to the right, smiling,* or *touching his nose with his hand.*

The *E* will then first determine its operant level over a period of time before he starts to reinforce. Here, changes in rate of response as a function of the reinforcement variables demonstrate conditioning. Such behavior is easily conditioned without awareness.

The *E* may decide in advance on a piece of *S*'s behavior he wishes to condition. In this case, he may choose something like *picking up a pencil, straightening his necktie,* and so on. If *E* chooses something as simple as this, he can usually afford to sit and wait for it to occur as an operant. If it does not, he may find it necessary to "shape" the behavior, as will be necessary if he chooses a relatively or highly unlikely piece of behavior, such as *turning the pages of a magazine, slapping the ankle, twisting a button, looking at the ceiling, placing the back of the hand on the forehead, writing a particular word,* or *assuming a complex posture.*

Many of the readers will question this use of the word "response." It is being used in accordance with the definition made explicit in Skinner's *The Behavior of Organisms:*

Any recurrently identifiable part of behavior that yields orderly functional relationships with the manipulation of specified classes of environmental variables is a response.

So far, this concept has proven a useful one: We have not explored the "outer limits" of the concept, with respect either to the topography or to the consequences of the behavior—we have not sampled broadly enough to find parts of behavior tentatively classifiable as responses that *didn't* yield such functions when we tried to condition them.

The contingencies of reinforcement, established in advance by *E*, determine the specifying characteristics of a response: he may reinforce only *one* word, or one trivial movement. In this case, he gets just that word or movement back from *S*. If *E* reinforces every spoken sentence containing any word of a specifiable class (e.g., *the name of an author*) he gets back from *S* a long discussion of literary figures. Plotted cumulatively, instances of naming of authors and titles in a whole sentence behave as a response class. By restricting reinforcement to naming *one* author, the discussion is narrowed. This method may serve fruitfully in research on what some call "response-classes" and others call "categories of behavior."

Procedures

Approximation conditioning ("shaping"). When the conditioning procedure begins, some *S*s will sit motionless and silent for some minutes after the instructions, but sooner or later they will emit responses that can then be reinforced. Such a period of inactivity, although trying to both *S* and *E*, does not seem to disturb the final results.

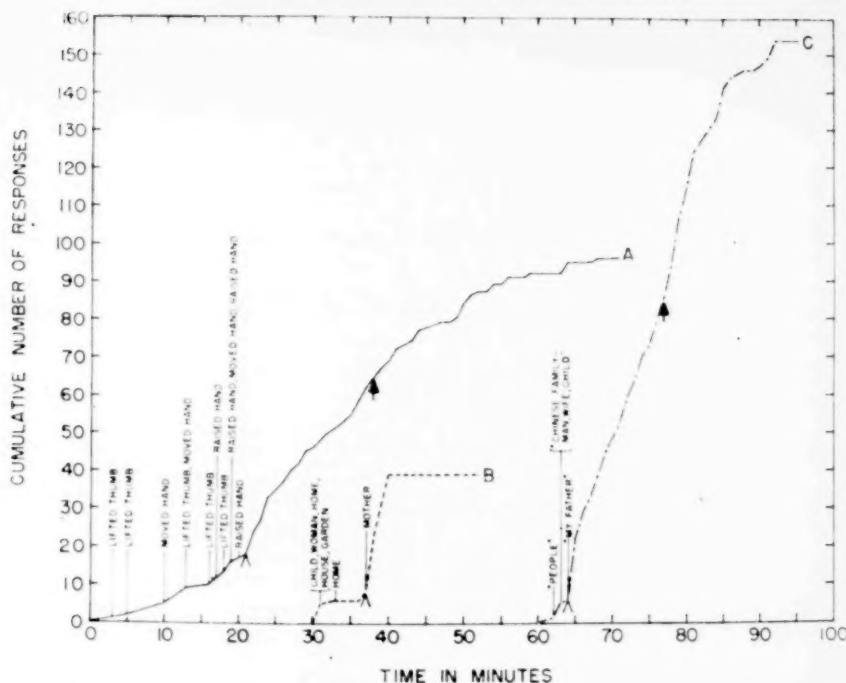If the response chosen has a high operant level, and occurs soon, *E* will

FIG. 1. APPROXIMATION CONDITIONING, FOLLOWED BY REGULAR REINFORCEMENT

Curves B and C shifted 30 and 60 minutes along X-axis respectively. During the approximation phase, the responses reinforced are named. Following the carat, only the specified R is reinforced.

(A) R: raising hand. S_R: pencil tap, recorded by S. Following arrow, no response is reinforced.

(B) R: saying word referring to member of S's immediate family. S_R: pencil tap, recorded by S. At sharp break, S says, "You've got me talking about my family, and it's none of your business," sits silent, and then changes subject and talks about other matters. In later inquiries, it developed that S had been worried about an alcoholic brother.

(C) R and S_R same as Curve B, another S. Following arrow, no response is reinforced.

proceed to regular reinforcement. Otherwise, he will find it necessary to "shape" the behavior, a process that is usually complete in 10, but may take as many as 25 minutes. In this procedure, E reinforces responses that are successively more and more like the response selected for conditioning[3] (Fig. 1a). This procedure re-

quires skill, and often bears a close (and not accidental) resemblance to the child's game of "Hot or Cold," and to the adult's "Twenty Questions," as well as to the procedure used in conditioning rats and pigeons. It is perhaps best explained by example:

Let us suppose that E had decided to condition a response such as *taking the top off his fountain pen and putting it back on again*. The E first reinforces the first movement he sees S make. This usually starts S moving

[3] The graphs presented were selected to give typical, rather than "pretty" results. They were chosen from among more than 60 such records. The problem of selection was that of deciding which to omit, rather than which to include.

about. Then he will reinforce successively *movement of the right hand, movement toward the fountain pen,* then *touching the pen, lifting it, taking the top off,* and finally *taking the top*
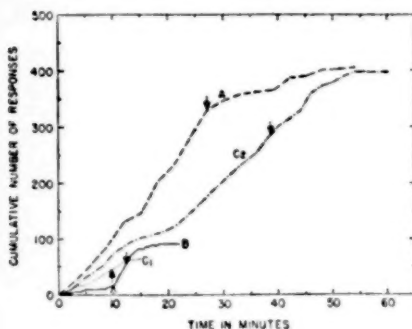

FIG. 2. REGULAR REINFORCEMENT AND SUBSEQUENT EXTINCTION

For Curves A_1, C_1, and C_2 at $t = 0$, reinforced response given for first time (approximation phase omitted). From $t = 0$ to arrow, regular reinforcement. Following arrow, no reinforcement.

(A) R: touching left hand to right ankle. S_R: "check" said by E; recorded by S.

(B) R: naming book title or author. S_R: pencil tap, recorded by S. Approximation conditioning to carat; then regular reinforcement of R only.

(C_1) R: folding hands. S_R: pencil tap, recorded by S. Verbalization occurs after maximal rate is achieved.

(C_2) same curve as C_1, with scales of both abscissa and ordinate multiplied by constants to yield a curve comparable to Curve A.

off and putting it back on. The effect of a single reinforcement in shifting, in narrowing down the range of a subject's activity, can be interesting to observe. It tempts E to depart from the procedure originally planned and to spend this time successively differentiating out more and more unlikely pieces of behavior.

It requires skill to shape behavior rapidly, whether one deals with rats, pigeons, or men. If E demands too much of S—that is, if he withholds reinforcement too long, S's responses

may extinguish. If E is too liberal—if he reinforces responses that are too similar to one another—he may condition these responses so effectively that further progress is slow. Responses that are conditioned as a result of E's lack of skill in spacing reinforcements, like those that are conditioned when E is sluggish in delivering reinforcement, are termed, in lab slang, "superstitious" responses. Even after they have been extinguished, and the correct response has been conditioned, they typically reappear during later extinction of the conditioned operant.

Verbal behavior is readily conditioned, and by the same techniques. Almost invariably, in this case, shaping is necessary unless S has been instructed to "say words," or unless the verbal response is saying numbers, such as "two," "twenty-five," and so on. When E shapes verbal behavior he should preselect verbal responses that can be unequivocally identified in a stream of language, as for example, saying *"aunt," "uncle,"* or *the name of any member of a family,* or *saying names of books and authors* (even in a particular field, as E chooses). By reinforcing sentences containing these words, E achieves control of a topic of conversation (Fig. 1b and c). The E may also bring S to say, and to say repeatedly, particular quasi-nonsense sentences, such as: "I said that he said that you said that I said so." The Ss may be conditioned to count, to count by threes, or backwards by sevens, and so on, when shaping is used.

Regular reinforcement (Fig. 2). Once the response occurs, E will first reinforce it regularly. One hundred regular reinforcements have proven ample to build up a resistance to extinction sufficiently great to permit E to shift S to most schedules without risk of extinction. (As Fig. 2 shows,

the number of R's in extinction is roughly proportional to the number of regular reinforcements in this range of values.) This number of reinforcements is also entirely adequate to yield a high and stable rate of response. "One-trial" conditioning, found in the rat and pigeon with comparable procedures, often shows itself: the rate of response assumes its stable value after a single reinforcement of the chosen response. When this occurs, S is not necessarily able to state what he is doing that yields him points.

Sometimes, an S, after giving a large number of responses under regular reinforcement, will show the symptoms of "satiation" (habituation?); that is, he will give negatively accelerated curves, declining to a rate of zero. Although reinforcements continue to be given, they bec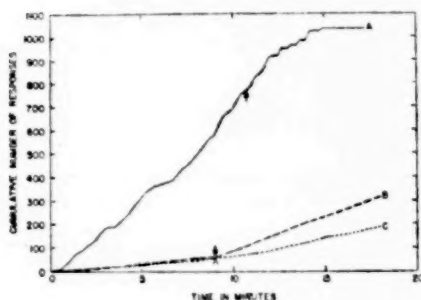ome progressively less effective. If S "satiates," E should simply say, "Keep earning points." This almost always restores the rate to its value before decline.

FIG. 4. INTERVAL SCHEDULES

(A) Variable interval (15 second average). R: Rub nose with right hand, S_R: "good," recorded by S. Five "goods" worth 1 cent to S. At $t = 0$, interval schedule begins after 75 sec. of regular reinforcement. At arrow, extinction begins.

(B) Fixed Interval (15 second). R: raise right forearm. S_R: pencil tap, recorded by S. At $t = 0$, interval reinforcement begins. At arrow, extinction begins. Note that extinction is like that usually obtained following fixed ratio reinforcement.

(C) Variable interval (15 seconds). R: raise right forearm. S_R: pencil tap, recorded by S. At $t = 0$, variable interval schedule begins. At arrow, extinction begins.

Schedules of reinforcement. The E is now free to follow any one of a number of schedules of reinforcement (**3, 4**), the simplest of which are fixed ratio (where every nth instance of a response is reinforced—Fig. 3) and fixed interval (where the first response occurring in each successive n-second interval following a reinforced response is reinforced—Fig. 4). The behavior observed under these schedules corresponds closely with that observed in lower animals. As with lower animals, E will find it impossible to shift directly to a high ratio of reinforcement, or to a long

FIG. 3. FIXED RATIO REINFORCEMENT

(A) R: slapping left knee S_R: pencil tap, recorded by S. Two reinforcements worth 1 cent to S. At $t = 0$, S is shifted from regular reinforcement to 15:1 rate of reinforcement. Extinction begins at arrow. Note bursts of high rates during extinction, followed by periods of complete inactivity.

(B) and (C) (2 subjects). R: raising left forearm. S_R: pencil tap, recorded by S. From $t = 0$ to $t = 9$, regular reinforcement (approximation phase omitted). At arrow, shift to 3:1 ratio of reinforcement, yielding increase in rate. Approximation phase data: for subject B, **9** reinforcements over 2 minutes; for subject C, **17** reinforcements over 5 minutes.

fixed interval, without evidence of extinction. Fixed intervals of 15 seconds and fixed ratios up to 6:1 may be established immediately without danger of extinction. When S is shifted, he will at first show great increase in the number of kinds of behaviors he exhibits, even though the conditioned R continues to occur at the expected rate. Verbal behavior increases greatly, too. If S has been working at a steady rate under regular reinforcement (for simple behavior, usually 15 to 25 responses per minute) when shifted to a short fixed interval schedule, he may exhibit counting behavior and state that he is earning a point every, say, fifth response. If variable-interval or variable-ratio schedules are followed it is necessary for E to have prepared in advance a program, guiding him in determining which one of a series of responses he should reinforce (for variable ratio), or after how many seconds he should reinforce a response (for variable interval).

The results obtained are typical: high rates of response occur under ratio schedules, and so does rapid extinction when reinforcement is withdrawn. Low, but stable, rates follow the interval schedules, with large, smooth extinction curves. Temporal discrimination, verbalized or not, may occur on fixed interval schedules. An exception is found in those cases where S behaves, on fixed interval, as if he had been on a fixed ratio—he may give an extinction curve appropriate in form to fixed ratio reinforcement. The schedule has not "taken over."

Discrimination. After following a schedule of reinforcement for a time, E may proceed to set up a discrimination, extinguishing the response in the presence of one set of stimuli and continuing to reinforce it on schedule in the presence of another (Fig. 5).
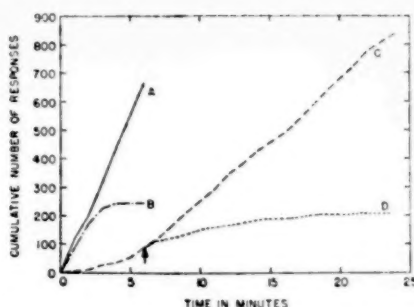
FIG. 5. DISCRIMINATION TRAINING

(A) (B) R: Rub nose with right forefinger. S_R: pencil tap, recorded by S. At t = 0, discrimination training begins, with S_D and S_Δ alternated through successive 30-second intervals and regular reinforcement in presence of S_D. No reinforcement with S_Δ. S_D: E's cigarette rests in ash tray. S_Δ: E's cigarette in his mouth. Curve A: responses under S_D; curve B: responses under S_Δ.

(C) (D) R: turning single page of magazine. S_R: pencil tap, recorded by S. At t = 0, regular reinforcement begins after approximation conditioning. At arrow, discrimination training begins, with S_D and S_Δ alternated through successive 60-second intervals, with regular reinforcement under S_D; no reinforcement in presence of S_Δ: desk lamp on; S_Δ: desk lamp off. Curve C: responses under S_D; Curve D: responses under S_Δ.

Here, it is advisable to have had S on a variable ratio or variable interval schedule, so that S's discrimination is not based solely on the omission of reinforcement. Discrimination in humans, as in rats, develops faster on ratio schedules.

For the rapid development of discriminations, it is desirable to choose as S_Δ (negative discriminative stimulus) a fairly conspicuous event, such as E putting (and keeping) a cigarette in his mouth, or putting his recording pencil down, or placing a book on the table and leaving it there, or crossing his legs. The use of less conspicuous S_Δ leads to the slow formation of the discrimination. Again, the data obtained are not readily distinguishable from the data obtained on rats (5),

except that the time scale is shorter; that is, the process is more rapid.

"Learning set" data may be obtained in an hour or so by repeatedly reversing a discrimination: the discrimination process occurs more and more rapidly with successive reversals.

Chaining. After having had S on a schedule of reinforcement, E may decide to chain two responses. He does this by conditioning the first (A) and then extinguishing it, simultaneously conditioning the second (B). When the second is conditioned, he will then proceed to withhold reinforcement until the first recurs. He may now make reinforcement contingent on the occurrence of the sequence A–B, and so on. The Es have succeeded in chaining together several responses by this procedure. Learning sets for chaining also occur in human beings; during the extinction of a simple operant, an S who had been conditioned to chain a series of responses and had then been extinguished regressed to these old responses and gave them in new sequences with each other, and with the response undergoing extinction.

Extinction. During extinction, E should be careful to show minimal changes in manner and behavior other than those necessitated by the failure to reinforce. By thoughtlessly putting down his recording pencil, E may obtain a very small extinction curve. Extinction curves obtained do not differ in any remarkable way from comparable data obtained on the lower animals.

The human shows many interesting incidental pieces of behavior during extinction, whether or not he is aware—i.e., has verbalized—that no more reinforcements are forthcoming. He will make statements to the effect that he is losing interest, that he is bored, that he has a pressing engagement; he may "get mad," or make mildly insulting remarks, or he may suddenly decide that "this is a stupid game," or a "silly experiment." He may indulge in conversation full of remarks deprecating institutions (e.g., the college) or himself. One S said, "I'm going to give you one more minute to give me a point, and if you don't, I'm going to go do math." (He left, in fact, after three minutes, when he had almost met the criterion of no responses in three minutes.) Many Ss volunteer the information that they "feel frustrated" when they can't get any more points.

The Ss also show behavior that some have called "regression"; that is, they give ("fall back on") responses that were reinforced during approximation conditioning. Regression is most easily demonstrated if E first conditions one response and then extinguishes it while conditioning another. In this case, during the extinction of the second response conditioned, S will usually shift back and forth between the two.

Recording

The essential records are the number of responses that occur in unit time, and the specification of the responses that were reinforced. If E makes a check on a piece of lined paper whenever the conditioned response occurs, moves down one line at the end of successive 15-second recording intervals and puts a bar across the check whenever a reinforcing stimulus is delivered, he will have a record from which the familiar graph of cumulative number of responses as a function of time can be constructed.

Recording should be done behind a screen such as that provided by a clip-board or book held vertically. This recording procedure, together with the fact that E is busy watching

S closely, has the merit that it is difficult for E to draw over-hasty conclusions about the "goodness" of the data that are being collected, so that badly intentioned student Es cannot manufacture "good" results. After learning to record with one hand, and to deliver reinforcements with the other, E will have no difficulty recording until S achieves very high rates of response, in which case, E may not be able to record or even count fast enough, and some responses will be missed. For experimental purposes, then, it is sometimes wise to choose a response for conditioning that requires not less than a second to complete.

The E may keep additional records such as, for example, a description of the responses reinforced during shaping, and of other behaviors that appear in extinction.

Awareness.[4] Let us define "awareness" as the disposition of S to verbalize one or more of the rules followed by E. The S may be partially or completely "aware"; that is, he may be

able to state one or more of these rules. He may be aware that a point is a reinforcing stimulus (as described in a textbook with which he is familiar); that E is trying to make him do something; that he is now doing something more often than he was before; that a certain response is being reinforced and is "right"; that he will get no points while E is smoking; that a point comes after every tenth response; that points average one per minute; that his response is being extinguished, and so on. He may be aware or unaware of any or of all of these.

Enough observations on such awareness have been made, both through the subject's emitted verbal behavior during the experiment, and by asking Ss about the experiment after its conclusion to permit some general statements:

1. In motor operant conditioning about half the subjects do not become aware of what response is conditioned, that is, of what they are doing that earns them points, until many reinforcements have been delivered and long after the stable rate of response has been achieved. These then, remain for some time blissfully ignorant of what they are doing. Conditioning and extinction may take place without S ever "figuring it out."

2. Very few subjects become aware of the particular schedule they are being reinforced on for many minutes. On fixed interval schedules some Ss show the beginning of a temporal discrimination long before verbalizing it, and others may verbalize the interval and only later show a corresponding gradual change in behavior.

3. Many subjects, particularly those whose motor behavior is being reinforced, keep up a running verbal commentary on the procedure, and

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[4] Since these experiments are most interesting when S is not aware of what behavior is being reinforced, we have followed the procedure of dividing laboratory sections into 2 groups, members of which will serve as Ss for the members of the other, and vice versa. One group is instructed to work on *verbal behavior*, and to bring it under discriminative control, and the other to work on *motor behavior*, and to study extinction as a function of one or another schedule of reinforcement. The effect of the sets established is sufficient for many Ss, despite their otherwise full information on human conditioning, to remain unaware of what E is reinforcing, and what procedures he is following.

Incidentally, it is futile to tell an S who has been conditioned, or who is familiar with conditioning that you want to "demonstrate conditioning," using him. Most Ss become very self-conscious under these circumstances and will not work. It is not at all difficult to demonstrate the procedure to a group that sits quietly and watches, but in this case it is necessary to use a naive S.

exhibit very elaborate "reasoning" behavior. Their behavior is not different from that of silent Ss, or of Ss who show a less "rational" approach to the situation.

4. Subjects will occasionally show insight or "aha!" behavior; i.e., suddenly state that *now* they know what gives them points. Others become aware gradually: "I think it has something to do with my chin." Some are never quite sure: "Look, what *was* right?"

5. Most important, with some exceptions that will be described, awareness seldom seems to alter the behavior. Sudden "insights," when they occur, are not necessarily associated with abrupt changes in rate; abrupt changes in rate, e.g., "one-trial conditioning," may occur *without* such awareness. Statements such as, "Oh, now you're extinguishing me," made by highly sophisticated Ss are not correlated with abrupt and permanent declines of rate to zero— S ("on the chance that I'm wrong, or that you'll change the procedure") proceeds to generate the remainder of a typical extinction curve.

6. In experiments where S is, by instruction, fully aware of the experimental contingencies (but where he does *not* know the kind of results he is "supposed" to give) he will behave *immediately*, on each instruction, as other Ss do only after long periods of reinforcement. He will immediately give, for example, a high rate of response when he is placed on fixed ratio; he will give only one or two responses under S_Δ, or in extinction, and so on. He has "learning sets," or, to put it another way, the instructions behave like well-established discriminative stimuli. He *starts* with the behavior that is asymptotic for uninstructed Ss.

In any event, the associated verbal behavior, whether or not it in any

way "directs" motor behavior, is highly sensitive to the experimental variables. During approximation conditioning and extinction, it is apt to occur at a relatively high rate, and to be aggressive in content: "The procedure is silly," E is "wasting my time," and so on. Under regular reinforcement, and on schedules, after stable performance is achieved, S has rather different sorts of things to say. The S's verbal "approach" to the situation is invariably interesting, as are the discrepancies between his statements about his performance and the performance itself. Since our procedures typically terminate with extinction, most Ss finally term the experiment "silly," "childish," and "stupid," despite the fact that they have "voluntarily" been working very hard indeed to earn points.

Possible research uses of the method. This kind of experiment, in which verbal behavior can be treated as either a dependent or independent variable, will perhaps find its greatest usefulness in the experimental analysis of so-called "cognitive processes" —that is, of S's awareness of what he is doing, and of the rather different dependencies of verbal and of other behaviors on a common set of experimental variables.

A second investigative area that this procedure makes amenable to experimental investigation is that of the classes of events that reinforce human behavior. What is a "point?" Why do "points" reinforce? How can their "value," i.e., their effectiveness in controlling behavior, be manipulated by instructions? Is a "point" from Experimenter A "worth as much" as one from Experimenter B? How will the addition of monetary reward vary the tendency of S to show satiation for "points"? What will showing S "group norms" for points collected do to his perform-

ance?   It offers the possibility of measuring *rapport*.

A third area is one on which some preliminary investigations have been done: the gross changes in behavior that occur in extinction and in shifting from one reinforcement schedule to one giving fewer reinforcements. Many subjects become, under these conditions, "disturbed," "upset," "emotional," "aggressive" and "frustrated." Observations of changes in the rate of speaking, of moving about in a chair, and of such idiosyncratic operants as scratching the head, tapping the forehead, and so on, suggest that these rates are a function of the ratio of reinforced to unreinforced responses (6).

Discussion

Operant conditioning as it was described in *The Behavior of Organisms* is concerned with the behavior that the layman calls "voluntary." This characterization is still valid— the behavior during conditioning is not "forced," as one might characterize the conditioned knee-jerk, or necessarily "unconscious," as might be applied to the conditioned GSR. *S*s work because they "want to." *S* 's behavior is nonetheless lawful and orderly as a function of the manipulations of *E*, and his behavior is predictable by extrapolation from that of lower animals.

These assertions, like the procedure itself, involve no theoretical assumptions, presuppositions, or conclusions about "what is going on inside *S*'s head." It does not assert that all learning occurs according to this set of laws, or that this process of conditioning is typical of all human learning. It does not assert that *S* is no better (or worse) than a rat, or that his behavior is unintelligent, or that since, say, *S*s get "information" from a reinforcing stimulus, so too do rats. The behavior is highly similar

in the two cases—we leave it to others to make assertions to the effect that rats think like men, or that men think like rats.

The procedures can be characterized as bearing close relationship to a number of parlor games.   Indeed, such conditioning might be considered by some as nothing more than a parlor game. This would not be the first time, however, that examples of rather basic psychological laws turned up in this context.   Parlor games, like other recreational activities are, to be sure, determined culturally, but it is doubtful that a parlor game could be found whose rules were in conflict with the general laws of behavior.

That the procedure is *more* than a parlor game is demonstrated by the fact that it provides a situation in which a number of the variables controlling voluntary behavior can be experimentally isolated and manipulated; that stable measures of a wide variety of behavior are yielded and, finally, that the procedure yields orderly data that may be treated in any one of a variety of theoretical systems.

Theoretical Discussion

The data lend themselves very well indeed to theoretical discussion in terms of "perceptual reorganization," "habit strength," "expectancy," or "knowledge of results," as well as to simple empirical description in the vocabulary of conditioning. *Chacun à son goût.*

Summary

A series of procedures are presented that enable an experimenter to reproduce, using the motor (and verbal) behavior of human subjects, functions that have been previously described in the behavior of rats and pigeons. Some remarks on "awareness" in the situation are made.

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SOME VERBAL MATERIALS FOR THE STUDY OF CONCEPT FORMATION

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This report presents materials developed for use in studies of verbal concept formation. It is not our intent to review materials used by others to study concept formation, as this has been done by Vinacke (3). However, it may be noted that tasks or materials which have been used are quite diverse in nature. With few exceptions (e.g., Weigl-type card sorting) no systematic series of experiments has been built around a single task. While this lack of task standardization attests to the ingenuity of individual workers in constructing new materials, the situation may not be entirely satisfactory for efficient development of laws and theories. In the more highly developed areas in psychology only a few basic tasks, procedures, or materials have been used. Thus, classical conditioning, the Skinner box, nonsense syllables, the pursuit rotor, and the psychophysical methods (to mention a few) all have had widespread use. While some may justifiably raise questions concerning generality of findings based on such a limited number of procedures and tasks, it cannot be doubted that interlaboratory communication and continuity is greatly facilitated by the use of common basic tasks and procedures.

The concept formation studies based on verbal stimuli are few in number (3). And, so far as we know, there have been no studies using verbal stimuli to which the already existent responses were known. We feel that an evaluation of the response tendencies of the Ss is extremely important. We have elsewhere (2) pointed out in some detail that concept formation basically consists of the perception of relationships among stimuli. Furthermore, we have suggested that relationships can be perceived only when one or more common responses are evoked by the different stimuli. If this is the case, and if verbal stimuli have certain response tendencies already established, then we need to know what these tendencies are. Therefore, our basic objective in developing materials for verbal concept formation was to determine what are the responses to the stimuli which are presented to S in a concept-formation situation. Thus, if we present the word *tomato* to S, we want to know what this stimulus makes S "think about." If we know such response tendencies for a large number of Ss for a large number of words, we can devise studies which will give us direct information on why different concepts are learned at different rates. And of course, the materials can be used in studying the influence of environmental and subject variables.

Our final set of materials satisfies the objective in a more restrictive way than we had originally hoped. Nevertheless, we have found these materials quite useful in several studies (recently completed or underway). It is possible that other inves-

tigators will also find them worthwhile for certain kinds of researches.

The method we used in developing these materials evolved only after trying several procedures which proved unsatisfactory for one reason or another. We shall first report briefly on these unsatisfactory procedures in order that it may be understood why we made the compromises we did. For want of a better term we call these procedures *scaling procedures*.

PRELIMINARY WORK

In both preliminary and final scaling we have worked only with concrete nouns. The Ss were always instructed to consider the words as nouns and never verbs, e.g., the word *fly* was to be thought of as the insect, not the act of flying. In our first attempt we tried to get (a) completely free associations to the nouns, and (b) successive free associations to each noun. We wanted the successive associations in order to determine the hierarchy of responses to each stimulus. This attempt proved unsatisfactory. We found very few stimuli which elicited the same response (a necessity, as indicated above) and successive responses to the same word seemed to be dependent on the previous responses as well as on the stimulus itself.

Our next step was to ask for a single word or phrase under essentially free-association instructions. We did make the instructions slightly restrictive in not allowing synonymlike words (for reasons which we will not discuss). We gave 120 nouns to 85 Ss under these conditions following which we set out to categorize the responses. The results were discouraging. Not only did we get few instances of identical responses to different stimuli, but in many cases

little identity of response to the same stimulus by different Ss. For 85 Ss we got as many as 50 different responses to the same stimulus. There was also considerable unreliability in categorizing the responses.

The third procedure involved a further restriction on the type of association we allowed Ss. Instructions told S that he was to give the first *descriptive* word or phrase which occurred to him upon seeing the stimulus word. The description could be in terms of physical properties or in terms of usage of the object symbolized. The instructions eliminated synonyms, clang associations, sequential associations, and several other types of responses which occur in free association. We gave 1,000 nouns to 84 Ss and again went through the categorizing procedure. While these instructions did yield the restriction in responses desired, we still obtained too few identical responses to different stimuli for our projected purpose and the categorizing remained somewhat unreliable.

However, the results of this third preliminary study gave us information which allowed us to plan what proved to be the final procedure. We noted that in the data for the third study the responses which overlapped (identical responses to different stimuli) were what we shall call "sense impressions." By sense impressions we mean such characteristics of objects as color, size, shape, texture, and so on. The response "yellow," for example, occurred to several different stimuli. Furthermore, such responses could be categorized with near-perfect reliability. Therefore, our final scaling technique was to use nouns to which we felt at least one sense-impression response would be fairly common. With these nouns we further limited our instructions so that

only descriptions in terms of sense impressions were allowed. We turn, then, to the details of the final procedure.

SCALING PROCEDURE

Materials and subjects. A total of 328 nouns was selected for the final run. Mimeographed data sheets were prepared with 328 spaces on them. Seven different groups of Ss were run, the groups varying in number from 13 to 30, for a total of 153. These Ss were all taking elementary psychology at Northwestern University. They constitute a sample from a population on which we expected to do the actual concept-formation experiments after the materials were prepared.

We felt it possible that Ss might develop more restrictive response sets than our instructions intended. That is, S might develop a set for responding with a certain class of responses, such as color responses, to a series of stimuli. This might continue until a color response was entirely inappropriate for a stimulus. He might then have a run on, say, shape of objects. We could not, of course, eliminate these runs if we were to present more than one stimulus to each S. Furthermore, for all we could tell, what appeared to be runs might actually represent the dominant responses irrespective of a run-set. In view of these considerations we presented the words in a different order to each of the seven groups, each order being determined on a random basis. If run-sets were operating they should not heavily influence the response frequency in a given category for any one word. Actually, as the data turned out, the response frequency for a given category for a given word was highly comparable among the seven groups,

so that we do not now feel that the run-sets were very strong.

Procedure and instructions. After instructions and preliminary practice (to which we will return in a moment) the 328 words were presented to Ss. The words were flashed on a screen and pronounced, one at a time, at a rate of 6 sec. per word. During the 6 sec., S perceived the word and wrote down the first association which occurred to him. Only a single word was allowed as a response. The Ss were warned not to rely on the auditory stimulus alone, but to look back at the screen before writing down a response. The rapid rate of presentation was intended to prevent S from doing any selecting of his responses. Rests were given after every 50 stimulus words. With instructions, practice, and main presentation, total experimental time averaged about 80 min.

The instructions, of course, are critical in our attempts to obtain the kind of responses we desired—sensory-impression responses. Yet, the verbatim instructions are of little significance since the establishment of the set came about largely through the discussion of responses to the practice words. The Ss were told that we were developing materials for use in concept formation or thinking studies. They were told that we were not interested in their "personalities," etc., and they could remain anonymous if they chose. Free association was then explained to them, although most were acquainted with the idea. They were next told that while we wished associations, only one particular class of responses would be allowed. These were then described as sense impressions, being the responses one might use to describe an object upon perceiving it for the first time. Then, a number of

illustrations of responses *not* allowed were given, e.g., synonyms, chain associations, opposites. Preliminary work with a small group showed that the instructions as outlined were insufficient; major success in establishing the set came only when we discussed responses made to practice stimuli.

Twenty practice nouns were used. The *S*s were shown the first practice word and were asked to write down as quickly as possible the first association which seemed to be a sense impression. Then we asked *S*s to announce the responses they had written. Responses which did not meet the requirements were discussed and *S*s were shown why these responses did not conform to the instructions. Thus, if someone responded to *acorn* with "oak" they were shown that "oak" was not a sense description of *acorn*. Responses such as "hard," or "small," or "brown" were noted as being sense impressions. However, we made a real effort to avoid "setting" *S*s for any particular class of sense impressions, for we were concerned about the run-sets mentioned earlier.

Following the presentation of the first word, and the discussion of the responses to it, the second stimulus was presented and the responses discussed. This continued until all *S*s appeared to "get the idea" and were responding as desired. This required approximately 10 words, varying somewhat from group to group. The remaining 10 practice words were presented without comment and at gradually increasing speed so that by the 20th word *S*s were responding at the rate required for the presentation of the major list. This speed, to repeat, was 6 sec. per word.

To many words one or more *S*s failed to give a response, a result

which we expected because of the rapid speed of presentation. Therefore, we do not have 153 responses to all stimuli. The actual numbers range from 147–153.

Categorizing. The categorizing of the responses was a fairly routine matter; in only a few cases was there doubt. Responses were combined if they apparently expressed the same idea. Thus, the responses "big" and "large" were assumed to mean the same thing. While we have not computed any index of reliability for the categorizing, it is noted that agreement of response frequencies for the various subgroups was high.

A total of 115 words in the original 328 elicited responses which we felt warranted their exclusion from the final list of stimuli. There was a number of reasons for exclusion, some of which we will mention:

1. Ambiguity of stimulus. For example, the word *dollar* was taken by some *S*s to mean a silver dollar, and by others to mean the bill.

2. Some stimulus words elicited such a wide variety of sense impressions that the frequencies in all categories were too small for subsequent use.

3. To some words sensory-impression responses were not given with sufficient frequency. For example, *S*s did not respond to the stimulus *tide* with any high proportion of sense impressions.

4. To some stimuli two or more dominant responses occurred which were somewhat contradictory. For example, to the stimulus *iodine*, both "red" and "brown" occurred frequently.

5. We eliminated most words which themselves are used to denote sense impressions, e.g., *gold*.

We have not rigidly applied the criteria indicated above; that is, we

do not have a "purified" set of responses. Those that we did retain which we recognized as not being unambiguous cases we felt would still be useful for some purposes.

Our final list contains 213 stimulus words. Our list will show the per cent of total responses to each word which fell into specified categories. In addition, for each word there is a miscellaneous category. If a particular response was given by less than 5 per cent of Ss, it was put in the miscellaneous category. Furthermore, we obtained some responses which were not sense-impressions (by our interpretation) and these were also put into the miscellaneous grouping.

In order to simplify the presentation it is necessary to code the response categories. There are 40 such categories. Responses not included in the 40 categories, but having a frequency 5 per cent or greater will be written out. Such instances are infrequent.

It may be noted (Table 1) that category 5 is indicated as "Smelly." We did not attempt to distinguish between good and bad or pleasant and unpleasant smells. Some people find gasoline smells pleasant, others unpleasant. But, the smell of an object, whether pleasant or unpleasant, is a descriptive term that is commonly used. We have found it useful in making lists of concepts to retain the idea of "smelly" although we realize that it is somewhat different than other words which may occur as descriptive terms for opposites, such as big and small, black and white, sour and sweet, etc. Finally, let it be clear that we do not claim that all response categories are "pure" sense impressions. It will be apparent in inspecting categories that there is some variation in this matter.

TABLE 1

CATEGORIES AND CATEGORY NUMBERS TO BE USED IN CONJUNCTION WITH TABLE 2

Category Number	Category	Category Number	Category
1	Round	22	Sour-bitter
2	Small	23	Hairy-furry
3	White	24	Wet-moist
4	Hard	25	Woody
5	Smelly	26	Strong, sharp tangy
6	Soft		
7	Shiny	27	Heavy
8	Big	28	Greasy
9	Long	29	Dirty
10	Yellow	30	Deep
11	Brown	31	Cold
12	Metallic	32	Noisy
13	Green	33	Fuzzy
14	Sweet	34	Light (not heavy)
15	Red		
16	Sharp	35	Square
17	Pointed	36	Clear
18	Slimy	37	Sticky
19	Black	38	Narrow
20	Smooth	39	Rough
21	Dark	40	Flat

RESPONSE FREQUENCY BY CATEGORY

In connection with Table 2, certain explanatory notes are essential. To the left is listed each stimulus word presented to the Ss. The response data are given under the column headed "Categories and Per Cent Frequencies." A number not in parentheses indicates the category, while the number in parentheses immediately following indicates the per cent of responses falling in the category. Different categories are set off by semicolons. Thus, if an entry reads 2(45); 6(22), it means that 45 per cent of the responses fell in Category 2 (small), while 22 per cent fell in Category 6 (soft). If a word appears instead of a category number it means that it is a special case not included in one of the 40 categories.

Next, there is a column headed "% Miscellaneous Responses." This

TABLE 2

(See text for explanation)

Stimulus Word	Categories and Per Cent Frequencies	% Misc. Responses	Thorndike-Lorge
Alley	21(49); 9(16); 38(14); 29(10)	10	13
Aluminum	7(59); 12(14); 34(12)	14	6
Ammonia	5(88)	12	5
Anchor	27(57); 12(15); 4(7); 8(5)	17	26
Ape	23(46); 8(30); 11(5)	19	6
Apple	15(67); 1(19); 14(5)	9	A
Armor	12(28); 7(25); 4(24); 27(14)	10	37
Asparagus	13(78); 9(9)	13	6
Asphalt	19(48); 4(29); 37(5)	19	5
Atom	2(87)	13	8
Auditorium	8(84)	16	3
Badge	7(32); 12(27); 1(21); 2(5)	15	8
Balloon	1(55); 34(17); Rubbery(8)	20	17
Banana	10(59); 9(12); 6(11); Slippery(5)	13	13
Bandage	3(73)	27	14
Barrel	1(72); 25(15); 8(6)	7	32
Baseball	1(70); 3(11); 4(10); 2(5)	4	15
Baton	9(50); 7(12); 12(7); Thin(11)	21	1
Beak	16(50); 17(17); 9(12); 4(9)	12	15
Bean	13(49); 2(18); 1(12); 9(6)	15	43
Bed	6(76)	24	AA
Beet	15(87)	13	11
Belly	1(43); 6(24); 8(8); 20(5)	19	10
Blood	15(91)	9	AA
Blush	15(96)	4	27
Bone	4(47); 3(34)	19	A
Boulder	8(46); 4(19); 27(10); 1(10)	16	5
Bracelet	7(25); 1(25); 10(19); 12(13)	18	10
Bread	3(35); 6(31)	28	A
Brick	15(46); 4(35); 35(7); 27(5)	8	49
Buckle	7(32); 12(31); 35(10); 4(9); 10(6)	12	8
Bungalow	2(46); 3(9); 25(7); Low(5)	33	8
Butter	10(62); 6(21); 28(7)	11	AA
Button	1(61); 2(15); 4(5); 3(5)	14	39
Cabbage	13(53); 1(15); 5(12); 3(5)	16	16
Cabin	2(39); 25(28); 11(11)	23	A
Camel	11(30); 23(15); 8(14); Humpy(20)	21	18
Canary	10(82); 2(5)	13	8
Capsule	2(51); 1(22)	27	2
Carrot	10(8); 9(6); Orange(69)	17	9
Cauliflower	3(64); 13(5); bumpy(5)	27	2
Cave	21(66); 30(6); Damp(14); Hollow(5)	9	33
Chalk	3(80)	20	13
Chamois	6(68); 20 (9); 11(8)	15	1
Cheese	10(43); 5(25); 26(6); Holey(6)	19	45
Cherry	15(77); 1(14)	9	35
Chestnut	11(47); 4(18); 1(14); 2(9)	11	16
Chocolate	11(61); 14(29)	10	20
Cigar	5(40); 11(26); 9(14); 1(7)	12	16
Cigarette	3(33); 9(15); 5(14); 1(6); Smoky(15)	17	22
Cinnamon	11(40); 14(21); 26(15); 5(6)	18	6
City	8(72); 32(5)	23	—

TABLE 2 (continued)

Stimulus Word	Categories and Per Cent Frequencies	% Misc. Responses	Thorndike-Lorge
Closet	21(64); 2(24)	12	20
Clove	26(32); 5(18); 13(8); 14(8); 11(7)	27	8
Coal	19(85); 4(7)	9	AA
Coffee	19(32); 11(24); 5(12); 22(11); Hot(12)	9	A
Collar	3(44); 1(16); Stiff(19)	21	44
Cork	34(27); 11(25); 6(21); 1(7)	21	11
Corn	10(81)	19	A
Cradle	25(29); 2(24); 6(11); 11(5)	31	21
Cranberry	15(69); 22(10); 14(7); 1(6); 2(5)	3	5
Crown	10(35); 7(20); 1(13); 12(7)	26	A
Crumb	2(79)	21	18
Crystal	36(52); 7(24); 4(9); 3(5)	10	27
Cucumber	13(52); 9(14); Prickly(5)	28	6
Custard	10(39); 6(26); 14(11); 20(7)	17	5
Daffodil	10(68); 5(12)	21	3
Dagger	16(70); 17(10); 12(6)	14	8
Dandelion	10(85)	15	6
Derby	1(33); 19(29); 11(14)	24	3
Diamond	7(65); 4(15); 36(9)	11	A
Diaper	3(50); 24(17); 35(8); 5(7); Triangle(5)	13	3
Dime	1(30); 12(23); 2(15); 7(13); Thin(9)	9	11
Dome	1(70); 8(5); 17(5); High(9)	12	15
Doughnut	1(71); 14(7)	22	5
Dungeon	21(67); Damp(22)	11	11
Earthworm	18(44); 9(17); 2(11); Crawly(5)	24	9
Eel	18(68); 9(15)	17	4
Elephant	8(83); Gray(11)	6	35
Enamel	3(28); 7(24); 4(20); 20(14)	14	8
Ether	5(70)	30	4
Eye	1(32); 2(10); 11(8); 7(6); Blue(26)	19	AA
Fang	16(75); 3(10); 17(5); 9(5)	5	4
Fishhook	16(70); 17(9); 12(5); 1(5); 2(5)	6	1
Flannel	6(54); 15(12); 33(5); Itchy(8)	20	9
Flea	2(86)	14	4
Forest	13(52); 21(14); 8(12); Dense(10)	12	AA
Formaldehyde	5(81)	19	—
Freckle	11(54); 2(19); 15(11)	16	8
Frost	31(54); 3(34)	12	41
Fur	6(75); 23(6); 33(5)	14	A
Garbage	5(80); 29(7)	13	14
Gardenia	5(65); 3(28)	7	1
Garlic	5(58); 26(25)	17	3
Gasoline	5(54); 24(7)	38	15
Germ	2(84)	16	10
Ginger	26(40); 11(15); 14(11); 5(11)	22	13
Globe	1(95)	5	41
Gnat	2(76)	24	4
Goat	3(29); 5(20); 23(18); 29(5)	29	4
Gorilla	8(42); 23(41)	16	3
Grape	1(18); 6(7); 14(5); 13(5); Purple(43)	22	34
Grapefruit	22(52); 10(23); 1(12)	12	2
Grass	13(88)	12	AA
Grasshopper	13(55); 2(18)	27	14
Gym	8(54); 5(21)	26	1

TABLE 2 (*continued*)

Stimulus Word	Categories and Per Cent Frequencies	% Misc. Responses	Thorndike-Lorge
Hailstone	4(49); 1(14); 3(9); 31(8); 2(7)	14	1
Hallway	9(54); 38(20); 21(16)	10	3
Harpoon	16(66); 9(16); 17(6)	12	1
Hatchet	16(77); 4(5); 12(5)	13	8
Head	1(66); 4(9); 2(5)	20	AA
Helmet	4(31); 12(22); 1(17); 7(8); 27(6)	16	21
Hog	8(20); 29(19); 5(15); Fat(26)	16	14
Honey	14(49); 37(29); 10(9)	13	A
Hospital	3(32); 5(23); 8(14); Clean(11); Quiet(5)	14	A
Icicle	31(45); 9(15); 16(14); 17(7); 36(5)	15	2
Ivory	3(65); 4(14); 20(12)	9	13
Ivy	13(83)	17	11
Jellyfish	18(49); 6(31); 2(5)	15	2
Jewel	7(67); 4(7)	26	41
Kitten	6(41); 2(25); 23(13)	21	35
Knife	16(84); 7(6)	10	A
Knob	1(68); 4(9); 2(5)	12	7
Knuckle	4(62); 1(11); Bony(8)	19	5
Lard	28(41); 3(27); 6(8); Thick(5)	13	9
Lawn	13(77); 20(8)	15	37
Lemon	22(65); 10(32)	3	27
Limousine	19(27); 9(26); 8(21); 7(14); Sleek(5)	7	2
Linen	3(59); 6(14); 20(9)	19	47
Lint	3(38); 2(32); 6(7); Flakey(8)	15	2
Lips	15(59); 6(24)	18	AA
Lizard	18(51); 13(23); 2(6); Scaly(8)	12	7
Mansion	8(83)	17	18
Manure	5(83); 11(7)	10	—
Measles	15(53); 2(5); Pimply(33)	9	3
Milk	3(83)	17	AA
Minnow	2(62); 18(16); 7(7)	15	3
Moccasin	6(53); 11(13); Leathery(11)	23	4
Moon	1(30); 10(20); 3(17); 7(13); 8(5)	14	AA
Moss	13(52); 6(22); 24(6)	21	22
Mouse	2(54); 23(7); Gray(27)	12	34
Mustard	10(75); 26(14)	11	7
Napkin	3(62); 6(12); 35(7)	19	16
Needle	16(53); 17(15); 2(9); 12(5); Thin(9)	10	34
Night	21(90)	10	AA
Ocean	8(33); 24(20); 30(12); Blue(24)	11	AA
Olive	13(62); 1(17); 19(7)	14	34
Onion	5(49); 3(15); 26(14); 1(9)	13	25
Oyster	18(46); 3(13); 1(9); 2(6); 6(6)	21	23
Pail	12(23); 1(18); 30(11); 7(10); 3(8)	30	16
Paste	37(64); 3(16); 18(7)	12	11
Peach	33(31); 10(20); 1(18); 14(17); 6(5)	10	29
Pear	10(44); 14(14); 1(9); 13(7); 6(5)	21	21
Pearl	3(37); 1(33); 7(12); 2(5); 4(5)	8	47
Pickle	22(44); 13(34); 14(7)	16	13
Pill	2(46); 1(28); 22(7)	19	7
Pillow	6(87); 3(5)	8	33
Pin	16(55); 2(22); 17(10); 12(5)	9	43
Pine	5(44); 13(25); Tall(8)	23	A
Pineapple	10(36); 14(22); 39(16); 1(7)	19	15

TABLE 2 (*continued*)

Stimulus Word	Categories and Per Cent Frequencies	% Misc. Responses	Thorndike-Lorge
Platter	1(38); 40(29); 8(10)	23	8
Pollen	10(32); 2(30); 5(5)	35	6
Pony	2(48); 11(14); 23(12)	27	32
Pot	1(29); 12(22); 19(13); 30(9); 7(7)	20	47
Puddle	24(61); 2(9)	30	3
Pup	2(50); 6(12); 23(11)	27	6
Rabbit	23(30); 3(25); 6(18); 2(6); Fast(10)	10	43
Rattlesnake	9(34); 18(28); 32(7)	31	3
Rhinestone	7(67); 4(10)	24	—
Rice	3(54); 2(24); 4(6)	15	AA
Rod	9(62); 38(9); 1(7); 4(7); 12(6)	10	44
Salt	3(53); 26(10); 4(7); Grainy(5)	26	AA
Sardine	5(30); 2(22); 18(20); Salty(5)	23	2
Saucer	1(59); 40(19); 4(5); 3(5)	12	7
Sauerkraut	22(41); 5(24); Stringy(17)	17	1
Scissors	16(78); 12(11); 17(5)	5	8
Seaweed	13(49); 18(28); 24(5); Stringy(11)	8	6
Sewer	5(61); 21(10); 29(8); 30(5)	15	7
Sheep	33(49); 3(23); 6(14)	13	A
Silk	20(41); 6(39); 7(6); Slippery(5)	9	A
Ski	9(38); 25(17); Slippery(9)	36	6
Skin	6(42); 20(17); 3(9); Pink(9)	23	AA
Skull	4(36); 3(22); 1(11); Bony(14)	17	12
Skunk	5(78); 19(14)	8	13
Snail	2(42); 18(18); 1(14); Slow(16)	10	8
Snow	3(71); 31(14); 6(8)	8	AA
Spear	16(68); 9(12); 17(12)	7	40
Spinach	13(90)	10	8
Spool	1(74); 2(9); 25(7)	10	7
Stadium	8(78); 1(7)	15	2
Stone	4(63); 2 (7); 1(6); 27(6)	19	AA
Straw	10(39); 9(11); Prickly(7); Thin(7); Brittle(5)	32	41
Sugar	14(82); 3(11)	7	AA
Sulphur	5(48); 10(36)	16	21
Tack	16(64); 17(16); 2(10)	10	9
Tar	19(52); 37(35); Thick(5)	8	9
Teeth	3(72); 16(8); 4(5)	15	A
Telephone	19(65); 32(19)	16	A
Thimble	2(37); 12(19); 7(15); 1(12); 4(9)	7	4
Tobacco	11(46); 5(23); 22(6)	25	36
Tomato	15(83); 6(7); 1(5)	5	11
Tongue	15(48); 6(11); 24(9); 9(8); 39(8)	17	A
Tunnel	21(54); 9(23); 1(6); 38(5)	12	22
Turpentine	5(67); 24 (6); 36(6)	20	4
Tweezer	12(26); 16(25); 2(17); 17(8); 7(6)	17	1
Velvet	6(67); 20(24)	9	32
Village	2(74)	26	AA
Vinegar	22(68); 5(14)	17	12
Waist	2(43); 1(24); 38(12)	20	33
Walrus	8(46); 19(13); 18(13)	28	2
Whale	8(77); 18(5)	18	8
Wheel	1(94)	6	A
Zoo	8(32); 5(30); 32(7)	31	2

is the percentage of responses which were not sense impressions or if they were, did not constitute a category of 5 per cent or more frequency. The last column, "Thorndike-Lorge," gives the frequency count of each stimulus word as listed by these authors (1). The numbers indicate the frequency of occurrence per million words. Thus, an entry of *17* indicates that this word occurs 17 times per million. An entry of *A* indicates that the stimulus word occurs between 50 and 100 times per million. An entry of *AA* indicates that the word occurs more than 100 times per million words.

COMMENTS ON USE OF MATERIALS

Construction and presentation of lists of concepts. It can be seen by inspecting Table 2 that a number of stimuli elicited a particular category of responses. Thus, the response category "round" was prompted by the stimuli *barrel, doughnut, cherry, dome, pearl, cabbage,* and so on. The frequency of occurrence of a particular category, of course, varies with the stimuli. In constructing a list of words for concept learning, the essential idea is to use several stimuli which have elicited the same response category. That is, one could use four words eliciting responses in the "round" category for the formation of one concept; four eliciting responses in the "red" category for another, and so on, until the list is as long as desired. In our initial studies we have used 24 stimulus words from which are to be formed six concepts. The number of words used as a basis for a given concept is, of course, quite arbitrary; we have used four but certainly this could vary. The stimulus words are randomized and the order varied from trial to trial. The *S* may indicate his grouping of

the stimuli in a number of ways. The simplest technique seems to be to ask him to name the characteristic which the four words have in common. The *S* responds with "round," or "white," or "large," depending upon the concept involved. The experimenter informs *S* after each response whether he was "right" or "wrong." Our lists have been presented at a 4-sec. per word rate; this gives time for *S* to respond, to be told "right" or "wrong," and for the experimenter to write down the response given.

"Validation" of scaling. We have completed an experiment which, in a sense, validates the scaling. We tested the obvious assumption that the more frequent the response common to the four words forming a concept, the more rapid the learning. We speak of this as *dominance level*; i.e., the higher the per cent frequency the higher the dominance level. For a given concept, we can use stimuli providing several dominance levels. For example, we have used three levels of dominance for each of the following concepts: *round, small, white, smelly, soft,* and *big.* For *white* the stimulus words for three levels of dominance are as follows (the numbers refer to per cent frequency with which "white" was given as a response):

High Dominance		Medium Dominance		Low Dominance	
Milk	83	Bone	34	Baseball	11
Chalk	80	Collar	44	Fang	10
Snow	71	Frost	34	Paste	16
Teeth	72	Lint	38	Sugar	11

Using three levels the results were as anticipated; the higher the dominance the more rapid the acquisition.

Use in studying interference effects. We believe that using these materials a number of studies can be done on

intralist interference effects in concept learning. Variations in amount of interference can be produced by varying the number and strength of competing response tendencies. If different stimulus words elicit the same response or responses, and if these stimulus words are not examples of the same concept, interference will result. Since we know the number and strength of responses to each word, systematic variation in response competition can be accomplished. If Ss are "set" by instructions for forming concepts based on sense impressions, the per cent frequency values should be accurate, hence relative interference strengths should be reasonably accurate. We also envisage studies on *interlist* interference, working on transfer, retroactive and proactive inhibition. Also, hypotheses which relate interference effects to certain other variables (such as anxiety) may find some use for these materials.

Some cautions. We do not as yet know all the pitfalls involved in using these materials for various purposes. But, we think it worthwhile to list two which seem to be important.

1. It should be clear that the per cent frequency values *do not necessarily* represent the relative strengths of responses for a given S. We obtained only a single response from S for each word. We know, say, that to a given stimulus 50 per cent of Ss responded with "white" and 20 per cent with "round." This does not necessarily mean that the 50 per cent who responded "white" would, if forced to give a second response, respond with "round" 20 per cent of the time. Our values give the per cent of Ss responding with a given

descriptive word, and that is all they give.

2. In constructing concept learning lists there are many factors which may need equalizing in some way. For example, suppose we want to determine whether color concepts are learned more rapidly than some other kind. In constructing the lists we should keep dominance level equal, number and strength of inappropriate response tendencies equal, interferences within each list, perhaps percentage of miscellaneous responses, and so on. Indeed, we have found that to form certain kinds of lists we do not have enough words among the 213 to accomplish what is needed by way of equalizing factors which might confound the results. We may soon find it necessary to scale more words.

SUMMARY

We have presented the results of an attempt to develop materials for use in the study of verbal concept formation. Our basic objective was to determine the frequency of response tendencies to certain verbal stimuli. If two or more words sometimes elicit the same responses, these words can be used as the basis for concept-formation tasks.

The words used were nouns. To each noun we obtained restricted associations. The restriction consisted of allowing S to respond only with sense impressions, such as size, shape, color, and so on. We have given the percentage of frequency of such responses to 213 nouns by 153 Ss, and have discussed some ways in which these materials may be used in concept-formation studies.

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A DISTRIBUTION-FREE TEST OF ANALYSIS OF VARIANCE HYPOTHESES[1]

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Rao (**3**, pp. 192–205) has shown that a chi-square statistic for a contingency table can be decomposed into components in much the same manner as a total sum-of-squares is decomposed in analysis of variance computations. By making a relatively simple modification of Rao's technique, it is possible to use this type of analysis in making a distribution-free (i.e., nonparametric) test of the hypotheses concerning main effects and interaction ordinarily tested by a two-way (or two-factor) analysis of variance.

Description of Test

1. The median value, Md, for the entire set of n observations is determined. This median should not be interpolated but should be determined only as a "boundary" which divides the entire set of observations, as nearly as possible, into two groups of equal size. In the remainder of this paper, n_a will represent the number of observations greater than or equal to Md and n_b will represent the number of observations less than Md.

2. A $2 \times r \times c$ contingency table is set up where r is the number of row conditions in the experimental design and c is the number of column conditions. The third "dimension" in this table corresponds to the division of the scores by Md. Thus, the frequency, $_b f_{ij}$, will represent the number of observation less than Md for the cell in row i and column j and $_a f_{ij}$ represents the number of observa-

tions in this cell which are greater than or equal to Md. Obviously,

$$n_a = \sum_i \sum_j {_a f_{ij}} \quad \text{and} \quad n_b = \sum_i \sum_j {_b f_{ij}}.$$

3. The total chi-square value, χ_T^2, can be computed from equation [1a] below if the numbers of observations for each cell of the $r \times c$ experimental design, $n_{ij} = {_a f_{ij}} + {_b f_{ij}}$, are all equal and if $n_a = n_b = n/2$.

$$[1a]: \quad \chi_T^2 = (4rc/n) \sum_i \sum_j ({_b f_{ij}} - n/2rc)^2$$

If $n_a \neq n_b$ but all n_{ij} are equal, equation [1b] below can be used.

$$[1b] \quad \chi_T^2 = \sum_i \sum_j \left[\frac{({_a f_{ij}} - n_a/rc)^2}{n_a/rc} + \frac{({_b f_{ij}} - n_b/rc)^2}{n_b/rc} \right].$$

Formula [1c] below is the most general expression and can be used without restriction on n_a, n_b and the n_{ij}

$$[1c] \quad \chi_T^2 = \sum_i \sum_j \left[\frac{({_a f_{ij}} - n_{ij} n_a/n)^2}{n_{ij} n_a/n} + \frac{({_b f_{ij}} - n_{ij} n_b/n)^2}{n_{ij} n_b/n} \right].$$

In all three formulas above, the expected frequencies (i.e., the terms on the right of the numerators) are obtained from the null hypothesis that the main effects and interaction effects produce no change in the distribution of scores. According to this hypothesis we should expect that the proportion, n_b/n, of the n_{ij} scores for each cell should be below Md and the

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proportion, n_a/n, should be above it. In all cases, χ_T^2 has $(rc-1)$ degrees of freedom.

4. The chi-square values of the row effects, χ_R^2, and the column effects, χ_C^2, are computed using the marginal totals of the $2 \times r \times c$ contingency table. If $n_a = n_b = n/2$ and all n_{ij} are equal, equations [2a₁] and [2a₂] below can be used.

[2a₁] $\chi_R^2 = (4r/n) \sum_i (_b f_{i.} - n/2r)^2$

where $_b f_{i.} = \sum_j {_b f_{ij}}$.

[2a₂] $\chi_C^2 = (4c/n) \sum_j (_b f_{.j} - n/2c)^2$

where $_b f_{.j} = \sum_i {_b f_{ij}}$.

If $n_a \neq n_b$ but all n_{ij} are equal, formulas [2b₁] and [2b₂] may be used.

[2b₁] $\chi_R^2 = \sum_i \left[\dfrac{(_a f_{i.} - n_a/r)^2}{n_a/r} + \dfrac{(_b f_{i.} - n_b/r)^2}{n_b/r} \right]$

[2b₂] $\chi_C^2 = \sum_j \left[\dfrac{(_a f_{.j} - n_a/c)^2}{n_a/c} + \dfrac{(_b f_{.j} - n_b/c)^2}{n_b/c} \right]$.

Formulas [2c₁] and [2c₂] are general expressions and can be used without restrictions on n_a, n_b and the n_{ij}.

[2c₁] $\chi_R^2 = \sum_i \left[\dfrac{(_a f_{i.} - n_i\, n_a/n)^2}{n_i.n_a/n} + \dfrac{(_b f_{i.} - n_i.n_b/n)^2}{n_i.n_b/n} \right]$

where $n_{i.} = \sum_j n_{ij}$.

[2c₂] $\chi_C^2 = \sum_j \left[\dfrac{(_a f_{.j} - n_{.j}n_a/n)^2}{n_{.j}n_a/n} + \dfrac{(_b f_{.j} - n_{.j}n_b/n)^2}{n_{.j}n_b/n} \right]$

where $n_{.j} = \sum_i n_{ij}$. In all three pairs of formulas above, the expected frequencies for the main effects are obtained for the null hypothesis that the distributions of scores are identical for all levels of the row or column conditions. Thus, for χ_R^2, we should expect the proportion, n_b/n, of each $n_i.$ to be below Md and the proportion, n_a/n, to be above it. In all cases, χ_R^2 and χ_C^2 have $(r-1)$ and $(c-1)$ degrees of freedom, respectively.

5. The chi-square value for the interaction effect, χ_I^2, is most easily computed by subtraction as in [3] below.

[3] $\chi_I^2 = \chi_T^2 - \chi_R^2 - \chi_C^2$

χ_I^2 has $(r-1)(c-1)$ degrees of freedom.

The general expression for χ_I^2 is fairly complex and is given by Rao (3, p. 103) in somewhat different notation. Recomputation of χ_T^2, χ_R^2 and χ_C^2 would appear to provide a faster check than computation of χ_I^2 from Rao's expression.

The expected frequencies for Rao's expression are obtained from the null hypothesis that the $_b f_{ij}$ and $_a f_{ij}$ for all cells can be predicted from appropriate marginal totals, in much the same manner as they are predicted in a chi-square test of independence in a contingency table. In other words, the hypothesis states that the row and column effects are independent.

6. The tests for the main effects and interaction are made by comparing the obtained values of χ_R^2, χ_C^2 and χ_I^2 with values from cumulative chi-square distribution for the appropriate degrees of freedom and significance level, α

Computational Example

Suppose that we have an obtained distribution of error scores in dial

reading for each of 16 subjects in each of 9 experimental groups. Also, let us suppose that three different dials, A, B, and C, and three different levels of illumination, 1, 2, and 3, were used in all possible combinations so there is a total of nine conditions, each of which was presented to a different group. Let the median of the combined distributions, Md, be 50 and suppose that we have the $2 \times 3 \times 3$ contingency table in Table 1.

Since all $n_{ij} = 16$ and $n_a = n_b = 72$, formula [1a] can be used to compute χ_T^2.

$$4rc/n = 4 \cdot 3 \cdot 3/144 = 1/16,$$

$$n/2rc = 144/2 \cdot 3 \cdot 3 = 8$$

$$\chi_T^2 = 1/4[(2\text{-}8)^2 + (4\text{-}8)^2 + (5\text{-}8)^2$$
$$+ (7\text{-}8)^2 + (9\text{-}8)^2 + (8\text{-}8)^2$$
$$+ (10\text{-}8)^2 + (13\text{-}8)^2 + (14\text{-}8)^2]$$

$$= 128/4$$

$$= 32.000$$

Formulas [2a₁] and [2a₂] were used to compute χ_R^2 and χ_C^2.

$$4r/n = 4c/n = 1/12,$$

$$n/2c = n/2c = 144/2 \cdot 3 = 24$$

$$\chi_R^2 = 1/12[(11\text{-}24)^2 + (24\text{-}24)^2$$
$$+ (37\text{-}24)^2]$$

$$= 338/12$$

$$= 28.168$$

$$\chi_C^2 = 1/12(19\text{-}24)^2 + (26\text{-}24)^2$$
$$+ (27\text{-}24)^2$$

$$= 38/12$$

$$= 3.188$$

Equation [3] was used to compute χ_I^2.

$$\chi_I^2 = 32.000 - 28.168 - 3.188$$

$$= 0.664$$

Since χ_R^2 has two degrees of freedom,

TABLE 1

Contingency Table for Dial Reading Experiment[a]

		Md = 50 Illumination Level			
		1	2	3	$_sf_i$
Dials	A	2	4	5	11
	B	7	9	8	24
	C	10	13	14	37
	$_sf_i$	19	26	27	72 $= n_b$

		Md = 50 Illumination Level			
		1	2	3	$_sf_i$
Dials	A	14	12	11	37
	B	9	7	8	24
	C	6	3	2	2
	$_sf_i$	29	22	21	72 $= n_a$

the row effect is significant beyond the 0.1% level. χ_C^2 with two degrees of freedom, is significant only at the 10% level and χ_I^2 is obviously not significant.

Small Expected Frequencies

Since r and c will be relatively small for most experiments (i.e., less than 10), pooling of cells to avoid small expected frequencies would probably result in a serious loss of information. In a recent article, Cochran (1) advises against pooling in any applications of the chi-square test and also concludes that the long accepted minimum expected values of between 5 and 10 are too conservative. Cochran also states that ordinary chi-square tables can be used with contingency tables with more than one degree of freedom where small ex-

[a] The data in this table are fictitious and are intended for illustrative purposes only.

pected frequencies are relatively few (about one in five or more) and where the minimum expected frequency is as small as one. If the contingency table has 30 or more degrees of freedom, an ordinary chi-square table can be used even if most expected frequencies are small and the minimum frequency is as small as two. Cochran gives references for exact tests for contingency tables where ordinary chi-square tables cannot be used. Also, Rao (**3**, pp. 201–205) gives exact tests which are relatively simple for small sample sizes. Therefore, it appears that small expected frequencies need not prevent use of the analytic technique described in in this paper.

Relationship to Mood Tests

Mood (**2**, ch. 16) discusses several extensions of the median test which are distribution-free tests of analysis of variance hypotheses. In his treatment of the two-way design with replication, Mood describes exact tests and chi-square approximate tests which are computationally similar to those described in this paper. However, his test for interaction effects is quite different and consists of making a series of iterative transformations of the scores until the medians of the transformed scores are zero for all rows and columns. After this is done, the interaction hypothesis is tested by computing a chi-square test of independence for a $2 \times r \times c$ contingency table whose third "dimension" corresponds to the differences in sign of the transformed scores. This test is obviously tedious and Mood states, without further qualification, that it is "very nearly but not quite distribution free" (**2**, p. 405). Thus, the test described in this paper appears to have considerable advantages in the treatment of interaction effects.

Use of Test with Experimental Designs with other than Two Factors

The analytic technique described in this paper may easily be used with a one-factor experimental design. If there are r conditions, a $2 \times r$ contingency table may be set up and a chi-square value for this table can be computed as in equations [2a₁], [2b₁] and [2c₁]. An essentially equivalent test is described by Mood (**2**, p. 398). If a two or more factor design without replication is used, the main effects of the design can be tested in this manner if the number of conditions for the other main effects is sufficiently large. Otherwise, an exact test could be used.

Extension of this test for use with designs of three or more factors, with replication, is also possible. The use of this test in analyzing a three-factor design, with replication, is also possible. The use of this test in analyzing a three-factor design, with replication, is described below.

1. The over-all median, Md, for the entire set of n scores is determined and a $2 \times r \times c \times b$ contingency table is set up where b is the number of blocks, set up in the same manner as in the analysis of the two-factor design. In this table, $_af_{ijk}$ represents the number of observations in the cell for row i, column j and block k which are less than Md and $_af_{ijk}$ represents the number of observations in this cell greater than or equal to Md.

2. A total chi-square value, χ_T^2, can be computed from a relatively slight modification of equations [1a], [1b] or [1c]. The most general form, which is modified from [1c], is given below.

$$[4] \quad \chi_T^2 = \sum_i \sum_j \sum_k \left[\frac{(_af_{ijk} - n_{ijk}n_a/n)^2}{n_{ijk}n_a/n} + \frac{(_bf_{ijk} - n_{ijk}n_b/n)^2}{n_{ijk}n_b/n} \right]$$

where $n_{ijk} = {}_a f_{ijk} + {}_b f_{ijk}$. χ_T^2 is distributed with $(rbc-1)$ degrees of freedom.

3. Chi-square values for the main effects, χ_R^2, χ_C^2, and χ_B^2 are computed in almost the same manner as for the two-factor analysis. The general equation for χ_B^2 is given below in equation [5].

[5]
$$\chi_B^2 = \sum_k \left[\frac{({}_a f_{..k} - n_{..k} n_a/n)^2}{n_{..k} n_a/n} + \frac{({}_b f_{..k} - n_{..k} n_b/n)^2}{n_{..k} n_a/n} \right]$$

where $n_{..k} = \sum_i \sum_j n_{ijk}$ and ${}_a f_{ijk} = \sum_i \sum_j {}_b f_{ijk}$. $\chi_R^{2'}$ and χ_C^2 may be computed as above with appropriate changes in the subscripts. χ_R^2, χ_C^2, and χ_B^2 are distributed with $(r-1)$, $(c-1)$ and $(b-1)$ degrees of freedom, respectively.

4. The total of the chi-square values for the four interaction effects, χ_I^2, can be computed by subtraction as in equation [6] below.

[6]
$$\chi_I^2 = \chi_T^2 - \chi_R^2 - \chi_C^2 - \chi_B^2.$$

If χ_I^2 is less than is required for significance for the interaction effect with the smallest number of degrees of freedom, the analysis may be stopped at this point. Otherwise, the significance of the interaction effects should be evaluated as in steps 5 and 6 below.

5. A $2 \times b \times c$ table, a $2 \times r \times b$ and a $2 \times r \times c$ table are obtained by summing the frequencies in the $2 \times r \times c \times b$ contingency table across rows, columns, and blocks, respectively. A χ_T^2 is computed for each of these tables by using formulas [1a], [1b] or [1c], with appropriate changes in subscripts, as in the two-factor analysis. Thus, a ${}_{BC}\chi_T^2$ is obtained for the

$2 \times b \times c$ table which was summed across rows, a ${}_{RB}\chi_T^2$ is obtained for the $2 \times r \times b$ which was summed across columns and a ${}_{RC}\chi_T^2$ is obtained for the $2 \times r \times c$ table which was summed across blocks.

6. The chi-square values for the three double interaction effects are computed as in the equations for [7] below. The pairs of main effects for which these interactions are being indicated by the left-hand subscripts.

[7]
$$\begin{aligned} {}_{BC}\chi_I^2 &= {}_{BC}\chi_T^2 - \chi_B^2 - \chi_C^2 \\ {}_{RB}\chi_I^2 &= {}_{RB}\chi_T^2 - \chi_R^2 - \chi_B^2 \\ {}_{RC}\chi_I^2 &= {}_{RC}\chi_T^2 - \chi_R^2 - \chi_C^2 \end{aligned}$$

${}_{BC}\chi_I^2$, ${}_{RB}\chi_I^2$ and ${}_{RC}\chi_I^2$ are distributed with $[(b-1)(c-1)]$, $[(r-1)(b-1)]$, and $[(r-1)(c-1)]$ degrees of freedom, respectively.

7. The chi-square value for the triple interaction effect, ${}_{RBC}\chi_I^2$, is computed as in equation [8] below.

[8]
$$ {}_{RBC}\chi_I^2 = \chi_I^2 - {}_{BC}\chi_I^2 - {}_{RB}\chi_I^2 - {}_{RC}\chi_I^2 $$

${}_{RBC}\chi_I^2$ is distributed with $[(r-1)(c-1)(b-1)]$ degrees of freedom.

8. The tests for the three main effects and the four interaction effects are made by comparing the obtained chi-square values with the values obtained from a table of the cumulative chi-square distribution for the appropriate degrees of freedom and significance level, α.

The analysis for the three-factor design indicates the general procedure for analyzing designs with more than three factors. In such analyses, a total chi-square value is computed for a contingency table where all conditions are represented and chi-square values are also computed for each of the main effects. If a potentially significant interaction effect is still seemingly possible, total chi-square

values are computed for all possible tables of two or more main effects in the full contingency table. The chi-square values for the interaction effects can then be computed by subtraction.

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Received April 4, 1955.

A METHOD OF ACTUARIAL PATTERN ANALYSIS

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A number of papers appearing during the last few years have considered the problem of analyzing patterns or profiles of psychometric test scores. Several indices of "profile similarity" have been proposed (**1, 2, 4, 7, 8, 9**). In a recent review, Cronbach and Gleser (**2**) show that these different indices may be all regarded as variants of the general Pythagorean formula for the linear distance between two points in n-dimensional space, where n is the number of scores in the profile, which itself is considered as a vector or point in the test space.

In this same paper, the authors make a point which is of the utmost importance and which undercuts the whole of this literature and renders even their own conclusions, while correct, essentially irrelevant. They point out that "*similarity is not a general quality. It is possible to discuss similarity only with respect to specified dimensions*" (italics in original) (**2**, p. 457). A pattern of test scores, such as an MMPI profile or a Rorschach psychogram, is thought to embody predictive validity with respect to a considerable number of different psychological dimensions of the testee. Two individuals may be similar in some of these dimensions and wholly dissimilar in others; *it is meaningless to speak of people or other complex entities as being "similar" without specifying some dimension(s) of comparison.*

If one is interested in similarities between test patterns, conceived as *geometrical configurations*, the relevant dimension of comparison is indeed measured by Cronbach and Gleser's D function. However, the psychologist is not interested in geometrical configurations but in people. If the MMPI profile embodies information concerning two independent psychological attributes, it is obvious that two such profiles, a given distance apart in the test space, may represent similarity with respect to one attribute and dissimilarity with respect to the other. Only by remote coincidence would a measure of the dimension of geometrical distance serve also as a metric for a psychological dimension, and a suitable index of similarity for one attribute could not, by definition, measure similarity in the other attribute if the two are truly independent. For the purposes of the psychologist, *no single index of profile similarity can be expected to have general utility* nor is it reasonable to expect that a measure of geometrical similarity will have *any* utility.

Two examples. The motivation behind this recent interest in the analysis of test patterns is the conviction of the clinician that not all of the psychological meaning of a profile of test scores can be abstracted in any linear combination of them. That is, the clinician believes that certain psychological dimensions, which we shall call the criteria, are nonlinear joint functions of the several tests in the profile. This is the only valid basis for the current emphasis on patterns as such. We shall consider two simple examples of such nonlinear relationships and illustrate (*a*) why measures of geomet-

rical distance are irrelevant to the problem, and (b) a general method for exploiting any nonlinear validity for estimating the criterion which may be in the profile.

Meehl (7) has demonstrated that two "yes-no" test items having zero validity with respect to a dichotomous criterion, could have up to perfect validity when considered jointly. This "paradox" of Meehl's may be seen to be a special case, for dichotomous distributions, of the function $y = x_1 x_2$, where y is the (continuous) criterion and x_1 and x_2 are the (continuous) predictors or test scores. The intercorrelations between the criterion and the tests individually may be zero and yet the criterion be entirely predictable from the scores taken together—from their "profile." For convenience, we shall assume all variables to be expressed in standard scores. The test space in this example is the $x_1 x_2$-plane. Suppose the criterion, y, increases *positively* with increasing absolute values of x_1 and x_2 in the $(++)$ and $(--)$ quadrants of this plane, and *negatively* with increasing absolute values of x_1 and x_2 in the $(+-)$ and $(-+)$ quadrants. Then the profile (3, 3) is psychologically equivalent to the profile $(-3, -3)$ but rather different from (1, 1). Geometrically, however, the patterns (3, 3) and $(-3, -3)$ are far removed from one another and (3, 3) is much more similar to (1, 1). The *only* psychological criterion for which similarity to the reference profile (3, 3) could be expressed by the Pythagorean D function would be one of the form $y' = (x_1 - 3)^2 + (x_2 - 3)^2$. Thus, none of the indices now available have any relevance to the psychological dimension of this example.

Consider another instance. A Rorschach expert might contend that some psychological attribute, say, emotionality, could be estimated from two Rorschach indices taken together as a pattern. He might suggest that, when the first index, x_1, is within average limits, the other index, x_2, is proportional to emotionality. But when x_1 is above or below average values, then x_1 varies as the inverse of emotionality. That is, $y = (x_2$ for $|x_1| < 2; 1/x_2$ for $|x_1| > 2)$ + Error of estimate. In this case, the pattern (3, 3) is psychologically equivalent to the pattern $(-3, 3)$, whereas the geometrical distance between these two points is considerable. (3, 3) is psychologically very different from (1, 1) although the configurations are geometrically close together. Note further that an index that would be appropriate as a metric for the first example would not do at all here. The points (3, 3) and $(-3, -3)$ which are equivalent for the function $y = x_1 x_2$, are very dissimilar with respect to the criterion of this second example.

It is the hypothesis of pattern analysis that psychological criteria can be validly estimated by nonlinear joint functions of sets of test scores. If several independent criteria can be estimated from the same set of scores, the joint function suitable for predicting one will not in general serve for predicting any other. This is the same as saying that any index which will measure similarity of (or psychological distance between) two persons on a given dimension will in general give spurious results in comparing the two persons (or profiles) on any other dimension. For the same reasons, of the indices thus far proposed (Cronbach and Gleser's D, Cattell's r_p, Du Mas' r_{ps}, Kendall's tau, Meehl's difference index), all of which measure various aspects of the *geometrical* distance between pro-

files, none are of any general use for the estimation of psychological criteria from test score profiles or for measuring *psychological* distance between profiles.

A solution. A simple, general method does exist, however, which will do both of these jobs. Consider again the predictor space defined by the test axes x_1 and x_2, expressed in standard scores. One wishes to predict a psychological criterion, y, from the "profile" formed by a pair of scores on the two tests (or to measure similarity with respect to the dimension y between given pairs of profiles, which is the same thing). To get the utmost predictive validity possible, one allows y to be *any* joint function of x_1 and x_2, i.e., $y = f(x_1, x_2) + $ Error of estimate. One obtains a sample of N profiles with a value of y associated with each as measured by some external criterion. The N profiles are plotted as points in the test space. The test axes are drawn orthogonal and intersect at $x_1 = x_2 = 0$ (see Fig. 1).

Suppose that the y values associated with the profiles in each quadrant of this space are averaged separately for each quadrant. If the function $f(x_1, x_2)$ is in fact $y = x_1 x_2$ as in the first example above, the y means for the $(++)$ and $(--)$ quadrants will be positive and approximately equal. The y means for the $(+-)$ and $(-+)$ quadrants will be negative and about equal. One is now in a position to say that profiles in, e.g., quadrants $(++)$ and $(--)$ are similar with respect to y and those in quadrants $(++)$ and $(+-)$ are dissimilar. Similarly, the best estimate for y for any profile in the $(++)$ quadrant will be the mean y value found for that quadrant.
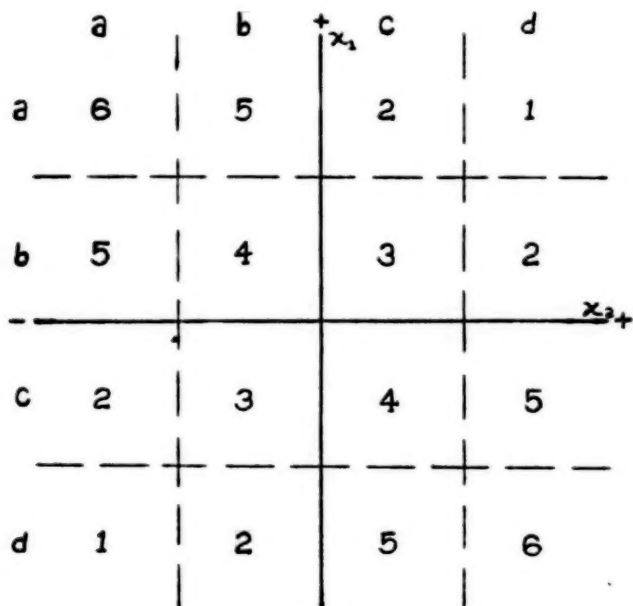
Such estimates would of course be

FIG. 1. RANK-ORDER OF y MEANS IN THE 16 CELLS OF THE TEST SPACE: $y = x_1 x_2$

extremely crude. To increase the precision of measurement, one partitions the test axes more finely, i.e., uses more cells in the test space. In the case represented in Figure 1, each axis has been partitioned at its quartiles, yielding a space of 16 cells. Again, the y means are calculated for each cell. The numbers entered in the cells of Figure 1 are the rank orders of size of the y means which would result if the function involved is $y = x_1 x_2$. Now one can say, for example, that profiles in cells ad and da are equivalent with respect to y and less similar to those in cell bc than are profiles in cell bd. Again, the best estimate of the *value* of y is the y mean of the cell in which the given profile falls. If another psychological dimension, z, is to be estimated from the profile x_1, x_2, one compiles a separate table of cell means for z in the same way. The degree of predictability of the criterion from the profile or set of scores is conveniently indicated by the statistic multiple-eta (6) and significance tests are also available.

This method can obviously be generalized to any number, n, of tests. If each test is partitioned into k intervals, there will be k^n cells in the test space and k^n entries in the actuarial prediction table containing the cell means. The method is applicable to categorical as well as continuous criteria, in which case the most frequent category of y occurring in a given cell is entered in the table instead of the y mean. The method may be used with categorical test data as well or even mixtures of the two. The categories of x_1, in this case, serve as the intervals for x_1.

This method requires sufficient entries for each cell of the test space so that the mean or modal y values obtained for each cell will be stable. Since there are k^n cells in the space,

this means that an exceedingly large sample N will be required to provide normative data for profiles composed of several tests if there are to be many intervals k, on each test axis. It can be shown, however (6), that with test reliabilities of the order presently available in psychological work, extremely coarse partitioning (e.g., four, three, even two intervals per axis) introduces relatively little additional error of measurement. Moreover, it usually happens that the tests in a profile are not independent of each other, in which case values will occur in many of the cells only rarely. These cells may be disregarded for practical purposes, thus considerably reducing the sample N required.

A PROGRAM OF EMPIRICAL PATTERN ANALYSIS FOR THE MMPI

To illustrate a practical clinical application of this method, we shall refer to the 9-scale profile of the MMPI. If one dichotomizes each scale axis, there result 512 cells in the test space. It will be convenient to number these cells from 0 to 511 and to code the profiles in such a way that the number of the cell to which a profile belongs is immediately given by its code. For scale values above the median, assign the code number *1* and for scale values below the median assign the code number *0*. The codes for the 9 scales of the profile are written in the usual scale order; thus the profile *48, 58, 52, 63, 53, 72, 65, 64, 53* would be coded *000101110*, if *60* were the median for each scale. This code may be read as a binary number, equal in this case to 46— this profile falls into cell 46 in the test space.

Over a period of time, profiles could be collected, coded, and filed under the appropriate cell designation to-

gether with whatever psychological data are available on the subjects producing the profiles. As data accumulate for the more popular cells, the psychological picture characteristic of these cells will emerge. An investigator interested in a particular dimension, y, can go to the files and compute y means for those cells for which sufficient data have accumulated. Multiple-eta (6) and the associated significance tests will tell him the extent to which MMPI profiles can predict that dimension; the table of cell means for y (or modes if y is qualitative) will be his predicted values for y for new profiles belonging to these cells; profiles belonging to cells having similar means he will regard as equivalent with respect to y. If the psychological data filed with the profiles does not include the y dimension, the investigator must gather a new sample. The files will help him to the extent of informing him which cells are sufficiently popular to warrant being sampled; e.g., if he wishes to predict y for college freshmen, he will ordinarily need data only on those cells in which profiles most commonly occur for this population.

If it is desired to increase the precision of measurement, the number of intervals, k, on each scale axis may be increased from 2 to 3. However, with 9 scales in the profile, this would result in over 19,000 cells in the test space. Although many—perhaps thousands—of the cells would be essentially empty in the population and could be ignored, this would still require an impracticably large sample on which to base the prediction tables. One solution would be to reduce the number of scales considered, either by eliminating the less discriminating scales or perhaps by using the first few principal component factors in their stead. Six scales with three

intervals on each would probably be a workable number.

Another solution would be to remain with the nine dichotomized scales and add as a tenth dimension a dichotomous index of general elevation, such as $(\sum_{i=1}^{9} x^2)^{1/2}$, coded 1 if above the grand scale mean and 0 if below it. This would double the number of cells in the test space and increase the precision of measurement considerably. (In the two-dimensional example above, where the quadrants formed the four cells of the test space, adding such an elevation index would amount to superimposing a circle on the test axes with its center at the origin. The four enclosed sectors of the circle would form four cells of the new test space and the four quadrant areas outside the circle would make up the total of eight. In the MMPI test space, there would 512 low-elevation cells within the elevation index hypersphere and 512 high-elevation cells outside it.)

The method proposed requires that profiles falling within a given cell be regarded as psychologically equivalent. At first glance, this would appear to entail the same fallacy inveighed against in the first part of this paper; viz., the assumption that all dimensions of psychological distance can be ordered to a continuum of geometrical distance. However, this is not really the case. First of all, it is clear that all the dimensions in question share the same zero point; identical profiles are zero distance apart geometrically and psychologically. Error variance in the tests composing a profile will cause the observed points to scatter in the test space around the point representing the "true" profile; psychologically equivalent profiles will in general be

observed to differ from each other by small amounts of *geometrical* distance. Thus, it can be seen that the D function measures something of psychological interest after all, namely, joint error of measurement in the profile. Therefore, the treatment of points within small regions of the test space as psychologically equivalent is not inconsistent with the arguments presented earlier. Moreover, error variance aside, only exceedingly complex functions will differ sharply between two adjacent small regions of the predictor space. One can only adopt the compromise approach of partitioning the test space as finely as is consonant with the reliabilities of the profiles and the available N.

SUMMARY

1. The "similarity" of test profiles, or of the people producing them, can be defined only in terms of specified dimensions of comparison.

2. An index of similarity which correctly orders profiles along one dimension of comparison, will not in general be appropriate for measuring any other, independent, dimension.

3. Existing indices of profile similarity order profiles as geometrical entities along some dimension of distance in the test space. Only by remote coincidence will any such index serve as a measure of any *psychological* dimension related to the test pattern or as an index of similarity of persons producing the profiles.

4. The hypothesis of pattern analysis is that various psychological variables may be best estimated by nonlinear joint functions of the tests composing a given profile. The function that will estimate one variable will not in general estimate any other.

5. Multiple-eta provides a simple, general method for analyzing complex joint functional relationships. Basically a curve-fitting technique, it requires the gathering of normative data and may be thought of as an actuarial method of pattern analysis. With this method, criteria and test data may be continuous or categorical. The method makes possible prediction of the criterion from the profile, measures the degree and significance of this predictability, allows for the assessment of similarity and dissimilarity of profiles with respect to the criterion dimension.

6. An illustrative possible application of this method to the MMPI in the form of a long-term cumulative research project is described.

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REMARKS CONCERNING WILLERMAN'S PAPER ON KENDALL'S *W* AND SOCIOMETRIC-TYPE RANKING

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The problem of adapting Kendall's coefficient *W* of concordance to sociometric-type ranking—or generally, to rank matrices in which the number of rankers and the number of ranked are equal, and in which the principal diagonal is blank—had already been considered by me in 1951 (2).

The adapted coefficient of concordance was designated *W'*, and the formula presented was

$$W'' = \frac{12S'}{n^3(n^2-1)},$$

in which

$$S' = S \frac{n^2}{(n-2)^2}.$$

This formula is identical with that given by Willerman if *S'* is replaced by *S*.

The advantage of using *S'* instead of *S* in the formula for *W''* becomes apparent when it is desired to take tied rank scores into account which are frequently present in matrices of this type. In that case the formula is corrected to

$$W'' = \frac{S'}{\frac{1}{12} n^3(n^2-1) - n \sum_{T'} T'}$$

where

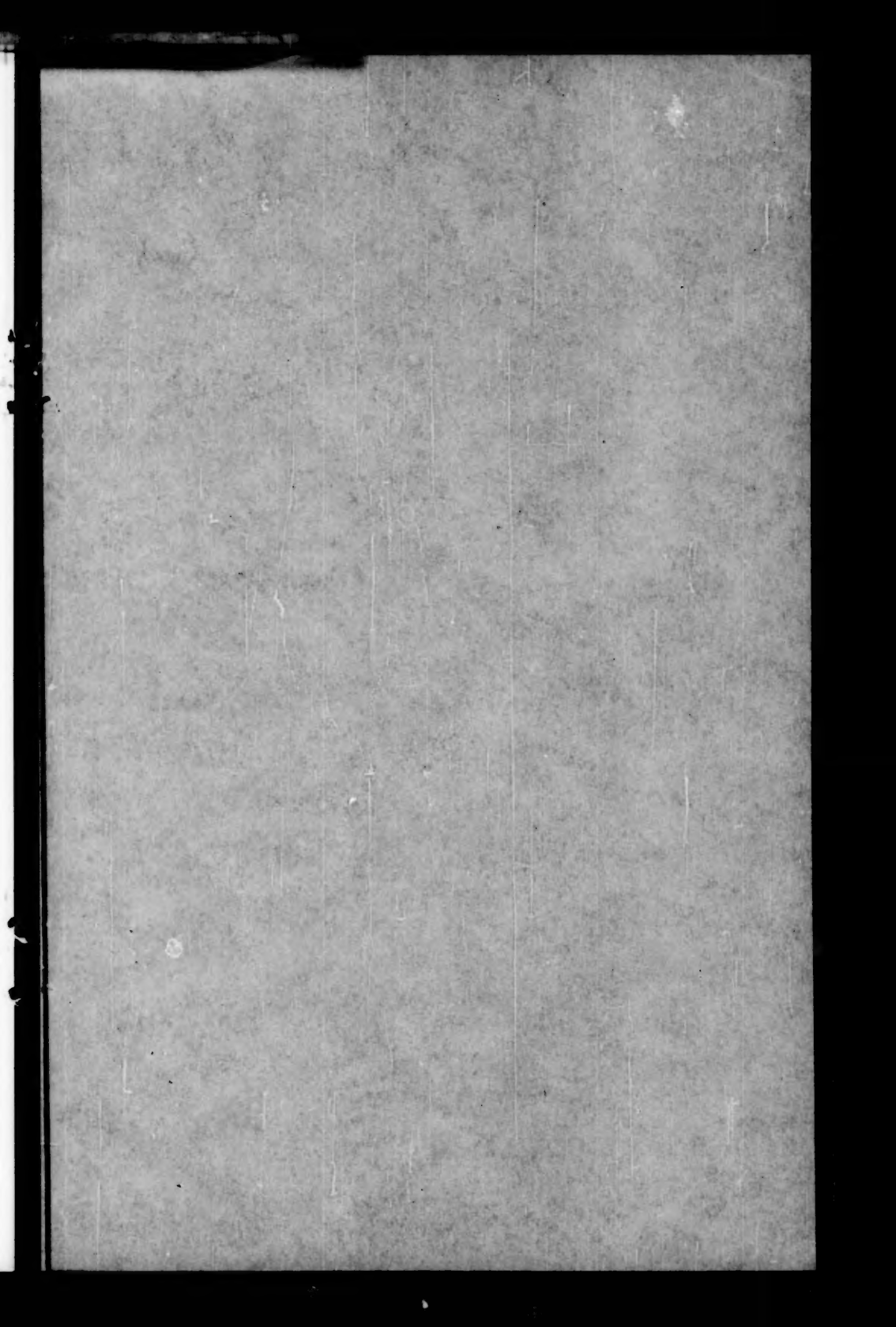$$T' = \frac{1}{12} \sum_t (t^3 - t)$$

and *t* denotes a set of ties in any one row.

This formula, though not its derivation, has recently been again presented in a paper of mine (3). It has been found useful in the study of small groups as an indicator of the reliability of the rankers and of the conspicuousness of the phenomena ranked. It was, for example, found that group members achieved significantly higher concordance values when judging group companions with regard to their dominance status than with regard to their popularity status. The respective *W''*-values were .72 and .57 in 20 small groups examined. This difference was statistically significant beyond the .01 level of confidence.

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Received April 18, 1955.